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EDINBURGH PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF
THE PROGRESS OF DISCOVERY IN NATURAL PHILOSOPHY,
CHEMISTRY, NATURAL HISTORY, COMPARATIVE ANATOMY,
PRACTICAL MECHANICS, GEOGRAPHY, NAVIGATION,
STATISTICS, AND THE FINE AND USEFUL ARTS,

OCTOBER 1. 1825 TO APRIL 1. 1826.

CONDUCTED BY

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PHILOSOPHICAL JOURNAL.

Art. I.—*On the Practical Construction of Achromatic Object-Glasses.* By Peter Barlow, Esq. F. R. S. Professor in the Royal Military Academy, Woolwich. Communicated by the Author.

1. A variety of methods have been proposed by mathematicians for determining the refractive and dispersive power of glass, and different principles have been given for computing the radii of curvature of the lenses composing the object-glass of our achromatic telescopes. The subject, indeed, is perhaps as well understood by theoretical opticians as can be desired; but this is far from being the case with many who are practically engaged in the construction of telescopes, and for their convenience only the present article has been written. It professes to throw no new light upon this highly interesting subject, but merely to bring under one head, and to reduce to the most simple form, all that is actually required by the practical optician, viz.

1. To determine, in the most accurate manner, the refractive index of his two glasses:

2. To determine their relative dispersive power:

3. To determine the radii of curvature of the different surfaces, so as to produce the achromatic property with the least spherical aberration.

With a view to the former of these, the instrument recommended and described by M. Biot, in his *Traité de Physique,* has been adopted, as well as the principle of calculation given by the same author; but this latter has been reduced to language more intelligible to general readers.

For the determination of the dispersive power, the instrument invented by Dr Brewster, and described by him in his "Treatise on New Philosophical Instruments," has been selected as the most simple, and as possessing every requisite precision for any practical application; and for the principles of computation, in this case, the formulæ, as given by Boscovich, and copied in the work last quoted, are those which, after some comparison, have been preferred.

Lastly, For computing the curvatures, we have taken, as decidedly superior to any other, the principles so ably illustrated by Mr Herschel in the Philosophical Transactions for 1821, and have extended his tables, in order to reduce the labour of computation to the least possible quantity.

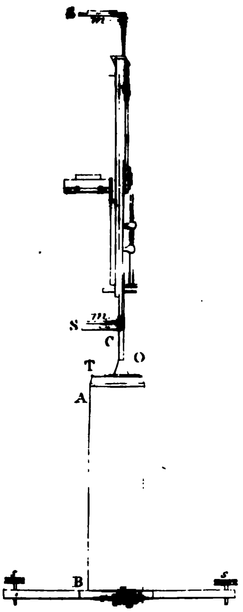
In every case, also, actual observations and calculations are stated in sufficient detail, to render the whole intelligible to every one who has any knowledge of the first principles of mathematics, and who is supposed to be required to construct an object-glass of any given focus, from specimens of flint and plate or crown glass, with whose properties, in the first instance, he is wholly unacquainted.

2. *Instrument for measuring the Angles of the Prisms, and for determining the Refractive Indices.*

The first thing requisite is, for the artist to form for himself two small prisms of the flint and crown glass he proposes working together, reducing them to an angle of about 30° each; but the exact measure of which must be afterwards determined by the instrument described below.

This is shewn in two elevations, Plate I. Figs. 1. and 2. Here *s s s* are three screws, which answer as feet to the instrument, and which at the same time serve for adjusting it to verticality. AB is a tube firmly attached to the centre of the three branches forming its base; T is an interior tube sliding into the former, and by [illegible] which the instrument may be turned in any positio[illegible] C is a sort of branch fixed to the interior tube, t[illegible] screwed the principal circle, graduated

Fig. 2.



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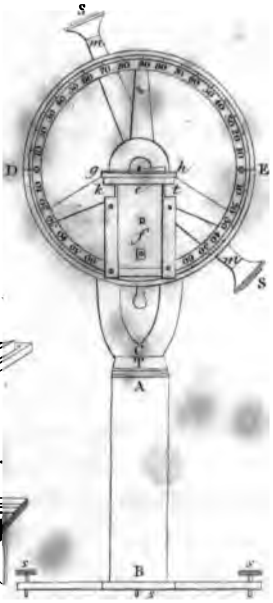

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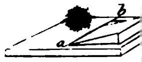

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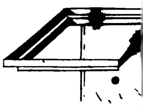

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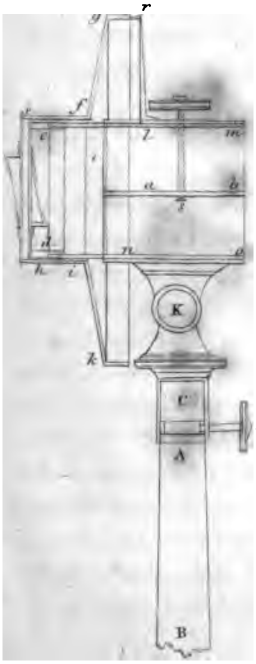

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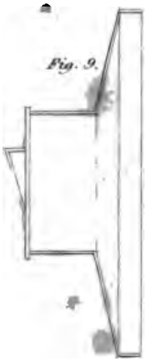

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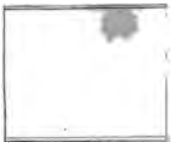

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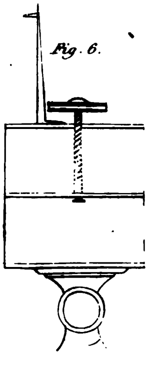

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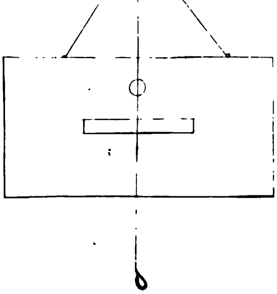

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as shewn in the figure: *m m* are two arms turning on one common centre, coinciding with that of the circle, each being furnished at its extremity with a disc, having an adjustable sight pierced with a fine hole: *e f* is a brass plate adjustable by the tangent-screw, seen in Fig. 2., and which plate carries at the upper part a square frame fixed at right angles to it. This square frame is counter-sunk on the inside, so as to receive a parallel plate of glass, on which the prism is placed, for observation, as seen in both figures.

The nature of this frame will be better understood by the perspective view of it, shewn in Fig. 3. Fig. 4. is a brass-plate ground parallel, and made to slide accurately over the frame, in such a way as to bring the straight chamferred edge *a b* exactly opposite to the centre of the graduated circle,—and the prism, when placed on the glass-plate, is brought exactly in contact with this line or edge. The tangent-screw mentioned above, serves to adjust the frame upwards or downwards, till the edge *a b* of the plate is opposite the centre, as above stated.

3. *To measure the Angle of the Prism.*

For this, it is best to use the parallel glass, blackened at the back, or to keep one glass for this purpose: lay this in the frame, and, by means of a short spirit-level laid upon it, adjust the instrument by the screws in the stand, till it is perfectly horizontal; then slip on the brass-plate, which ought also to be blackened, to prevent any confusion of reflected light. Now, bring both arms of the instrument above the horizontal line or zero, and set them both by means of the verniers to the same angle; as, for example, 40° or 50°, &c. Then looking through one of the small holes in the sights, the reflection of the other ought to be seen bisected by the edge of the brass-plate, which will shew the instrument to be correct; and if this should not happen, it must be brought to do so by adjusting the sights accordingly. This being done, lay on the prism, placing its sharp edge gently against the edge of the plate above mentioned, and then, while one of the sights remain fixed, move about the other, till the reflection of the small hole in the former is seen bisected by the straight edge as before, and then half the difference in the two readings will be the angle sought. This operation, which is very simple, may be repeated at several angles, and the mean result taken for the angle of the prism.

4. The following examples will sufficiently illustrate this operation.

Flint Prism, No. 1.

Fixed sight.	Moveable sight.	Difference.	½ Difference or Angle.
40° 0′	89° 38′	49° 38′	24° 49′
30 0	79 36	49 36	24 48
35 0	84 40	49 40	24 50
36 0	85 38	49 38	24 49
25 0	74 38	49 38	24 49
	Mean angle,	-	24 49

Plate Prism, No. 1.

Fixed sight.	Moveable sight.	Difference.	½ Difference or Angle.
40° 0′	89° 42′	49° 42′	24° 51′
35 0	84 40	49 40	24 50
30 0	79 44	49 44	24 52
25 0	74 44	49 44	24 52
20 0	69 40	49 40	24 50
	Mean angle,	-	24 51

The principle of this deduction is too obvious to call for any farther remark, than merely to state, that it is founded on the known law, that the angle of incidence is equal to the angle of reflection.

5. *Observations for determining the Index of Refraction.*

It is a known principle in optics, that, in the passage of light out of one medium into another, as, for example, from glass into air, the sines of the angles of incidence and refraction are to each other in a constant ratio; and this ratio is what is called the *index of refraction*.

In order to determine the data requisite for ascertaining this index, we must proceed as below.

Having adjusted the instrument as before, place in the frame the cle[illegible]llel plate of glass, instead of the blackened one used in the [illegible] l apply the blackened brass plate as before; bring [illegible] the prism in contact with the edge of the plate, [illegible] he last observations.

The sights must now be placed as shewn in Plate I. Fig. 1. viz. the one towards the edge of the prism, above the zero or horizontal line, and the one towards the base below the same, and the lower the better, setting it to some certain reading, as, for example, 60° or 55°, &c.

Place on the table, under the lower sight, a piece of clean white paper, and reflect upon it (if necessary) a strong light; bright sunshine is to be preferred. Then move about the upper sight till the eye perceives the refracted image of the lower sight bisected by the straight edge, and note its reading. These are all the data requisite for commencing the calculation; but, for the sake of greater security, it will be best to repeat the observation under three or four different incident angles.

The image seen in this experiment will be coloured and elongated, but there will still, with a little practice, be no difficulty in bisecting it.

Note.—In order to prevent any confusion in the computation arising out of the signs of the cosines above and below 90°, it will be best to register the supplements of the actual readings, or what they want of 180°, instead of the readings themselves.

6. *The following are a set of Observations on the above prisms.*

Flint Prism, No. 1.

No.	Supplement to reading of Lower Index. (Q.)	Supplement to reading of Upper Index. (P.)	Half the difference. (*d.*)	Angle of Prism as above found. (*a.*)
1	120° 0′	104° 10′	7° 55′	24° 49′
2	125 0	108 45	8 7	
3	130 0	113 0	8 30	
4	135 0	117 10	8 55	

Plate Prism, No. 1.

No.	Supplement to reading of Lower Index.	Supplement to reading of Upper Index.	Half the difference.	Angle of Prism as above found.
1	120° 0′	106° 0′	7° 0′	24° 51′
2	125 0	110 38	7 11	
3	130 0	115 0	7 30	
4	135 0	119 40	7 40	

Let the angle in the first of the columns be denoted by - Q
in the second by - - - P
in the third, or the half difference, by - d
And that in the fourth, or angle of prism, by - - a

Then the rule for computing the index may be stated in words at length as follow:

7. *Rule for computing the Index of Refraction.*

1. To the angle P add the angle d, and subtract $\frac{1}{2}a$ from the sum, and call the remainder = A.
2. Add $\frac{1}{2}a$ and d together, and call the sum = B.
3. Add together cotangent $\frac{1}{2}a$*, tan A and tan B; subtract 20 from the sum, and find the angle of which the remainder is the tangent, and call it = D.
4. From D subtract $\frac{1}{2}a$, and call the remainder = E †.
5. From cos Q subtract cos E, and find the natural number answering to the remainder as a logarithm, and it will be the index sought ‡.

Operation.	*Flint Prism.*	*First Observation.*
To P	= 104° 10′	To $\frac{1}{2}a$ = 12° 24′$\frac{1}{2}$
Add d	= 7 55	Add d = 7 55
From sum	= 112 5	Sum B = 20 19
Subtract $\frac{1}{2}a$	= 12 24$\frac{1}{2}$	
A	= 99 41	

Cot $\frac{1}{2}a$ = 12° 24′ -	10·6578454
Tan A = 99 61, or 89 19	10·7679350
Tan B = 20 19 -	9·5684856
Tan 84° 13′ = D -	10·9942660

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* In all these cases the log tan, &c. is to be understood.

† If, in any instance, the angle A should be less than 90°, then, instead of the angle D, as found above, we must take its supplement, or what it wants of 180°, in order to find E.

‡ The algebraical expression for this rule, which will be more intelligible than the abov [redacted] uainted with analytical subjects, may be expressed as below,

$$\tfrac{1}{2}\,a \tan(P + d - \tfrac{1}{2}a)\tan(d + \tfrac{1}{2}a)$$

$$\text{ndex } r = \frac{\cos Q}{\cos(D - \frac{1}{2}a)}.$$

See Biot, *Traité de Physique.*

From D = 84° 13′
Take $\frac{1}{2}a$ = 12 24
E = 71 49

From cos Q = 120°, or 60 } 9·6989700
Take cos E = 71 49′ - 9·4942361
Nat. N° = 1.6019 = Index 0·2046339

8. *Operation.* *Flint Prism.* *Second Observation.*

To P = 108° 45′
Add d = 8 7
From sum 116 52
Subtract $\frac{1}{2}a$ 12 $24\frac{1}{2}$
A = 104 27

To $\frac{1}{2}a$ = 12° 24′
Add d = 8 7
Sum B = 20 31

Cot $\frac{1}{2}a$ = 12° 24′ 10·6578454
Tan A = 104 27 or 75 31 } 10·5889079
Tan B = 20 31 - 9·5731227
30·8198760
Subtract 20·0000000
Tan 81° 23′ = D - - 10·8198760

From D = 81° 23′
Take $\frac{1}{2}a$ = 12 24
E = 68 59

From cos Q = 125° 0′ or 55 0 } 9·7585913
Take cos E = 68 59 - 9·5546581
Nat. N° = 1·5993 Index - 0·2039332

This differs from the former index by ·0026, and is given as an instance of extreme aberration; no greater difference than this can be allowed; should it ever exceed this quantity, the observation should be repeated. In a great number of such experiments I have generally found a complete agreement in the first three places of decimals:

The third line of observations gives Index = 1·6013
The fourth - - - - - = 1·5994
The first - - - - - = 1·6019
The second - - - - - = 1·5993

4) 6·4019

Mean Index, 1·6005

Similar operations for the plate prism give for a mean index $r = 1.5279$.

9. *Instrument for measuring the Dispersion, and for determining the Dispersive Ratio.*

It is a well known optical fact, that light, in passing from one medium to another, is not only refracted, but is decomposed into different coloured rays, thereby forming a spectrum, and that the extreme red ray is the least refracted, and the extreme violet the most. The indices of refraction for these two extremes are therefore different, and the difference between these indices divided by the mean index *minus* 1, is called the ***Dispersive Ratio***; and the ratio between the dispersive ratio for two different species of glass, is called the ***Ratio of the Dispersive*** powers, or ***Ratio of Dispersion***. This, also, is sometimes called the dispersive ratio of two glasses.

The instrument for determining this ratio may be described as below:

AB, Fig. 5. is a brass pillar, on the top of which fits the cap C, surmounted with a joint K; to the upper part of which is fixed a short tube $l\ m\ n\ o$, open on the side $a\ b$, having a set screw s. Within this short tube is inserted another tube of about [illegible] ngth, and which, when brought into any required [illegible] be fixed there by the set screw s shutting the exterior tube close upon it. This tube projects to the line *c d*, which shews its termination. *e f h i* is another tube which slips over *c d*, and carries at its end *f i* the circular plate *g k*, graduated on its outer edge from zero both ways to 180°; *v* is a vernier attached to the first outward tube *l m n o*. The diameter of these tubes may be about $2\frac{1}{2}$ inches. The end of the tube *e f h i* has an end or base at *e h*, in which is a circular hole about $1\frac{1}{2}$ inch in diameter, and against this there is a means of fixing a prism, as shewn in the figure. The tube *c d* is also terminated at *c d* with a similar end for the same purpose, but is made to slip out and in like a common diaphram, for the convenience of fixing the prism on the inside, in order that the interior faces of the two prisms may be parallel.

The construction of this instrument will be better understood by referring to Figs. 6, 7, 8, 9, 10, where Fig. 6. is the case-tube fixed to the stand, with its vernier and set-screw; Fig. 7. is the next tube inserted into this; Fig. 8. is its diaphram for carrying the prism inserted into Fig. 7.; and Fig. 9., is a short tube with a graduated circle, which fits over Fig. 7., and which also carries a prism, as seen in Fig. 5., where the several tubes are all in their places.

This instrument being thus provided, we must next get a piece of smooth board, about 2 feet square, well blackened with lamp-black, across which is to be stretched a parallel strip of very white clean card-paper. This is to be hung up, with the card-paper horizontally, in a good light, with a plumb-line passing across it as in Fig. 10. Then set up the dispersive instrument in front of it, at the distance of about 6 or 8 feet, and every thing will be ready for observation.

10. *Method of Observing.*

1. Remove the tube and graduated circle Fig. 9., with its prism, which is always to be that possessing the greater dispersion of the two, and turn the tube Fig. 7. about in Fig. 6. till the edge of the prism fixed to its end is upwards and perfectly horizontal, which will be known by the eye perceiving the plumb-line directly above the edge of the prism, and the refracted image of the same in the prism in one vertical line. For which

purpose a space is left open above the prism in the face of the diaphram. This being done, make it fast in this position by the set-screw. Remove the plumb-line, and looking at the card-paper strip, its upper edge will be seen strongly tinged with violet and blue, and the lower edge with red and yellow. Now, put on the tube and prism, Fig. 9., placing the base of this prism upwards and horizontal, and then, on examining the strip of card-paper again (the latter prism being the stronger of the two in producing dispersion), the upper edge will be found tinged with red and yellow, and the lower with the violet. If now, Fig. 9. be gradually turned round either to the right or left, while the eye is still regarding the card-paper strip, the colours on both edges of the paper will diminish, and, at length, in a certain position, will wholly disappear. This being well and carefully observed, register the reading shown by the vernier on the graduated circle above mentioned. Then turn the circle back in the other direction till the colours again disappear, and again register the reading shewn by the vernier: call half the intercepted arc between the two readings M. (This will be the difference of the readings, if both are on the same side of zero, but the sum if on different sides).

Let this observation, which is very simple, be repeated several times, and the mean of all the results taken for the value of M.

11. *Computation for the Ratio of Dispersion.*

1. Let the prism fixed in Fig. 8. or the fixed prism, and which we here suppose to be the plate glass, be called A, and let this letter also denote its angle; and let the flint prism in Fig. 9. be called B, which may also denote its angle. Then,
2. To the log sine of angle A, add the log of its index of refraction; and from the sum substract the log of the index of the refraction of B, and find the angle, of which the remainder is the log sine, and call it angle *a*.
3. To the log tangent of angle B, add the log cosine of angle M. and find the angle of which the sum is the log tang [illegible] angle *b*.
4. Fro [illegible] nd call the remainder $= c$.

5. From the log tangent of *c* subtract the log tangent of *a*; considering the remainder as a logarithm, find its natural number, and subtract that natural number from unity.

6. Now, multiply this remainder by the index of refraction of prism A, and by the index minus 1 (or the decimal part of the index) of prism B. Multiply also the index of the refraction of B by the decimal part of the index of A; lastly, divide the former product by the latter, and the quotient will be the ratio of dispersion between the two glasses.

Or, Add the logs of the three former numbers together, and the logs of the two latter, and the difference found by subtracting the latter from the former will be the log of the ratio sought [*].

Note.—It is assumed in the preceding rule, that the prism B owes its higher dispersion to its greater dispersive power, the angles being nearly equal; but with a less dispersive power (by having a greater angle), its dispersion may still be greater than prism A. In this case, the same rule will also obtain; only in the part numbered (5) in the above rule, we must add the natural number to unity instead of subtracting it; the reason of which will be seen in the algebraical formula.

12. *Example.*

Shewing the results of observation and calculation on the two prisms Plate No. 1. and Flint No. 1., of which we have already determined the angles and indices, viz.

Angle of Plate prism A = 24° 51′ index = 1.528 [†]
Do. Flint B = 24 49 index = 1.601

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[*] The analytical expression for this rule is,

$$\text{Sin } a = \frac{r \cdot \sin A}{R} \tan b \cos M \tan B = \tan b$$

$$\text{Dispersive ratio} = \frac{r\,(R-1)}{R\,(r-1)} \left\{ \tan\,(b-a) \cot a + 1 \right\}$$

r being the index of refraction of A, and R that of B.

[†] Three places of decimals are quite sufficient, and we have taken these to the nearest figure; both a little in excess.

Observation for finding angle M.

Readings with the index turned to the right, when the colour disappeared.		Readings with index turned to the left.	
	5° 10′		117° 44′
	5 16		117 54
	5 30		117 30
	5 20		117 44
	5 16		117 40
	5)25 92		5)588 32
Mean	5 18	Mean	117 42
			5 18
			2)112 24
		Angle M =	56 12

Then, by the rule,

Sin A = sin 24° 51′ = 9.6235016 to tan B = 24° 49′ = 9.6650346
Add log 1.528 = 0.1841234 add cos M = 56° 12′ = 9.7453056
9.8076250 tan 14° 25′ = b = 9.4103402
Sub. log 1.601 0.2043913
Sin 23° 39′ = a 9.6032337

From angle a = 23° 39′
Take angle b = 14 25
angle c = 9 14

From tan c = 9° 14′ = 9.2110184
Take tan a = 23 39 = 9.6414036
Nat. $n°$ = .37121 1.5696148

From 1.00000
Take .37121
Remainder, .62879

Log of remainder .62879 = 1.7985056
Log of index A 1.528 = 0.1841234
Log of [illegible]f index B .601 = 1.7788745
[illegible]irst sum, 1.7615035

Log of index B = 1.601 = 0.2043913
Log of decimal of index A = .528 = 1.7226339

Second sum, 1.9270252

From 1.7615035
Take 1.9270252

Nat. *n*° .68309 = 1.8344783

Whence the ratio of the dispersive powers of the two glasses is 1 : .68309; or .68309, according to the common mode of expression.

We have thus obtained the requisite data for determining the radii of curvature to be given to our plate and flint lenses, in order to produce an achromatic object-glass.

13. *Computation Tables, &c. for finding the Radii of Curvature; the refractive index of each glass, and their dispersive ratio, being given.*

If we were now merely required to correct the object-glass for colour or dispersion, all that would be necessary, would be to make the focal lengths of our two lenses in the direct ratio of their dispersive powers; and, therefore, with three of the surfaces formed at pleasure (at least within certain limits), the fourth might still be so determined as to produce a correction of colour; and this is probably still practised by some opticians: but the correction of colour is by no means all that is to be considered in working an object-glass for a good telescope; for if, also, we have not regard to the spherical aberration, the image, although free from colour, will be seen in a cloudy or smoky field of vision, which will render it very imperfect and indistinct.

With a view to this latter correction Mr Herschel has given a very elaborate and valuable paper in the Philosophical Transactions of the Royal Society, Part ii. for 1821, with Tables, &c., so as to reduce very considerably the labours of computation; and, by extending these tables to a greater length, it is presumed that we have added our mite towards the simplifying this important but otherwise laborious and intricate calculation. However, before entering upon an explanation of this process, it is proposed to give, in words at length, some preliminary rules for determining the foci of simple lenses, when the refractive power and radii of curvature are given, or the converse: for, notwithstanding these rules may be familiar, in some form or other, to practical opticians, yet, as we should wish this paper to contain every rule requisite in the construction of an object-glass, we shall, it is hoped, be excused for introducing them in a concise form in this place.

14. *Rules for determining the Focal Length of Lenses of given curvature* *.

1. *To find the focal length of a double convex lens for parallel rays, the radii of curvature and the index of refraction being given.*

Rule.—Multiply the two radii together: then add the two radii together, and multiply their sum by the decimal part of the index of refraction. And the former product, divided by the latter, will be the focal length.

Example.—The radii of curvature of a flint lens being 4 inches and 10 inches, and its refractive index 1.601, required the focal length.

$$\text{Here}\quad \left.\begin{array}{r}4\\10\\\hline 40\end{array}\right\}\ \text{and}\ \left\{\begin{array}{r}4\\10\\\hline 14\\.601\\\hline\end{array}\right.$$

8.414)40.000(4.75 inches focal length.

2. *When the two radii are equal, the rule becomes more simple, as follows.*

Divide the radius of curvature by double the decimal part of the index for the focal length.

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[*] The algebraical formulæ embracing all these rules may be stated as follows: viz.

For parallel rays, $f = \dfrac{r\,R}{a\,(r + R)}$

See *Encyclopædia Metropolitana*,—Optics.

Where f is [illegible] the decimal part of the index of refraction, and r and R the [illegible] both positive when both surfaces are convex, and negative w[illegible]

Example.—The radius of the two equal surfaces of a flint lens (whose index is 1.600) being 10 inches; required its focal length.

Here .600
2
———
1.200)10.000(8.33 focal length.

The same two rules hold good when both surfaces are concave, only then the result must be considered as negative.

3. *To determine the same for parallel rays in a plano-convex lens; the radius of the convex side and the index being given.*

Rule.—Divide the radius of curvature by the decimal part of the index of refraction, and the quotient will be the focal length.

Example.—Required the focal length of a plano-convex crown lens; the radius of curvature 12½ inches, and the index of refraction 1.520.

12½ = 12.5
.52)12.50(24.04 inches focus.

4. *To determine the focal length of a lens having one concave and one convex side, the radii and index of refraction being given, and the rays parallel.*

Rule.—Multiply the two radii together; multiply also their difference by the decimal part of the refraction: then the former product, divided by the latter, will be the focal length; which will be positive when the concave radius is the greater of the two, but negative when it is the lesser.

Example.—Find the focal length of a flint lens, the radius of the convex side being 10 inches, and of the concave 16 inches, the index 1.600.

10 16
16 10
—— ——
1st product 160 Difference 6
.600
———
2d product 3.6)160(44.44 focal length.

The result here is positive; but had the convex side been 16 and the concave 10, the focal length would have been the same, but the rays would have diverged, or the result would have been negative.

5. *Having the focal length of a double convex or plano-convex lens given, as also the negative focal length of a double concave lens, or of a concavo-convex lens, to find the focal length of the combined object-glass.*

Rule.—Multiply the two focal lengths together; divide the product by their difference, and the quotient will be the focal length of the compound object-glass.

Note.—If the negative focal length be the lesser, the resulting focus will still be negative, but if greater, it will be positive, and the rays will converge.

Example.—The focal length of a double convex lens is 6 inches, and of a concavo-convex lens 9 inches, negative. Required the focal length of the compound object-glass, formed by combining the two.

From negative focus =	9	9
Subtract positive focus =	6	6
	3	54

3)54

18 focal length required.

From these rules are drawn several others which may be of frequent use; for example,

6. *The index of refraction and one of the radii of a double convex lens being given, to find the other radius, so as to produce a given focal length.*

Rule.—Multiply the proposed focal length, the decimal part of the index (a), and the given radius together, for a dividend; and subtract the former part of this product from the given radius, for a divisor. Then divide the dividend by the divisor, and the quotient will be the other radius.

Exar [illegible] The index of a piece of flint-glass is 1·600, and one of [illegible] is to a radius of 10 inches. What must the oth [illegible] focus of 12 inches?

$$\begin{array}{rl} a, = & .600 \\ \text{Focal length,} = & 12 \\ \cline{2-2} & 7{\cdot}2 = \text{first product} \left\{ \begin{array}{l}\text{from } 10 \\ \text{take } 7{\cdot}2 \\ \hline \end{array}\right. \\ \text{Given radius,} & 10 \\ \cline{2-2} & 72 \text{ dividend.} \qquad \text{Divisor, } \underline{2{\cdot}8} \end{array}$$

7. ***The index of a piece of glass being given, to find what the equal convex surfaces must be to produce a given focal length.***

Rule.—Multiply the focal length by double the decimal part of the index, and the product will be the radius of the equal surface.

Example. With a similar piece of flint to the above, What must be the equal radii of its two surfaces to give a focal length of 6 inches?

$$\begin{array}{rl} \text{Here twice } a = & 1.2 \\ \text{Focus } = & 6 \\ \cline{2-2} & 7{\cdot}2 \text{ inches radius.} \end{array}$$

For a plano-convex lens we must multiply the decimal part of the index by the focal length for the radius.

8. ***The index of refraction and the convex surface of a concavo-convex lens being given, to find the radius of the concave surface, so that the lens may have a given negative focal length.***

Rule.—Find the dividend exactly as in Rule 6. Then add the first product to the given radius for a divisor. Divide the dividend by the divisor for the radius sought.

Example. The radius of the convex surface of a concave convex lens is 12 inches; the index of refraction 1.600; and the negative focal length is to be 5 inches. Required the radius of the concave surface.

$$\begin{array}{lr} \text{Focal length} & = 5 \\ \text{Decimal part } (a) & = {\cdot}6 \\ \hline \text{First product} & = 3{\cdot}0 \\ \text{Given radius} & = 12 \\ \hline \text{Dividend} & 36 \end{array}$$

$$\left.\begin{array}{l}\text{To } 12 \\ \text{add } 3\end{array}\right\} = 15 = \text{divisor.} \qquad \left.\begin{array}{r}36 \\ 15\end{array}\right\} \frac{36}{15} = 2.4 = \text{radius sought.}$$

9. *The index of refraction and radius of concave surface being given, to find the radius of the convex surface, so that the lens may have a given negative focal length.*

Rule.—Find the dividend exactly as in Rule 6. Then subtract the given radius from the first product for a divisor. Divide the dividend by the divisor, and it will give the radius required.

Example.—Let the numbers stand as in the last example, except that the given concave radius is 2·4 inches, and let the other radius be computed.

$$\begin{array}{rr} \text{First product} = & 3{\cdot}0 \\ \text{Given radius,} = & 2{\cdot}4 \\ \hline 3{\cdot}0 - 2{\cdot}4 = \quad {\cdot}6) & 72 \\ \hline \text{Radius sought,} = & 12 \text{ inches.} \end{array}$$

Remark. In a similar manner, the radii of curvature of a lens being given, and its focal length found by experiment, its index may be computed with great accuracy. On this subject some observations will be found in the concluding part of this paper.

(To be continued.)

Art. II.—*General Reflections on various important subjects in Mineralogy.* By Frederick Mohs, Esq., Knight of the Order of Civil Merit, Professor of Mineralogy at Freyberg, Fellow of the Royal Society of Edinburgh, of the Wernerian Natural History Society, &c. Continued from Vol. XIII. p. 218.

We shall not in this place inquire what kind of information, relative to the products of the mineral kingdom, should be excluded from Mineralogy; but it is necessary for us to examine whether, by proceeding solely upon the observation and comparison of the natural-historical properties of simple minerals, we may [illegible] something which, besides containing information of [illegible] same kind throughout, also possesses the other properties of a science. And here, although the words 'natural history,' and 'natural-historical properties,' have been frequently made use of, and will be used hereafter, it might still remain a problem to be solved, whether or not this science be Natural History.

In reference to this matter, the first object with which we have to occupy ourselves, is to examine these properties *by themselves*, and not in conjunction, as several of them occur in minerals. This will enable us to obtain a correct idea of them, to judge of their merits, and to apply them usefully; and for this purpose they must be disposed in a certain order, and designated by appropriate expressions. They are here explained as natural-historical, and not as physical, properties; that is to say, they are exhibited only in so far as they are applicable to Natural History, their explanation as physical properties forming part of Natural Philosophy. This explanation, as being a general preparation to the farther development of the science, is also necessary in Zoology and Botany; and it is called the *Terminology*, because it contains, besides the general investigation of those properties, also the explanation of the expressions, which are henceforth, for the sake of precision and perspicuity, to be used in a determinate and peculiar sense. A department of Geometry, analogous to terminology in Natural History, is that devoted to definitions; and here there are none of those difficulties with which we have to struggle in Natural History, because empirical ideas are totally excluded. Hence it is a particularly favourable circumstance in *mineralogical* terminology, that geometrical determinations may be received in it, the influence of which even extends beyond the limits of terminology, and confers so high a degree of evidence upon the idea of the species, one of the most important general ideas of the science, that in this particular it has evidently obtained a great advantage over Botany and Zoology. It is a property of every simple mineral to assume a regular form, that of *crystals*, whenever it becomes solid, and no external impediments have existed during the progress of its formation. *Crystallography*, therefore, is that part of terminology, in which it is possible to introduce mathematical considerations in the investigation of the natural-historical properties of minerals. This is the most important part of terminology; nay, we may safely maintain, that, without this property, mineralogy itself could not exist as a science,—that is to say, it could not form part of Natural History. Crystallography has been considered by naturalists from so many different points of view, that it is perhaps worth while to examine what it should be, as a part of *mineralogical terminology*. Every one who has for any time been occupied with the examination of minerals, must have, no doubt, observed, that certain minerals possess certain crystalline forms, while others are excluded from them. If we distinguish between simple and compound forms, we discover that the varieties of one and the same species assume various, sometimes a great many, simple forms; and that the compound forms in which they likewise occur, contain either these simple forms themselves, or such as are in a certain connexion with them, dependent not only upon the kind, but also upon the relative dimensions of the forms. Crystallography is not intended to investigate the reason *why* the property of assuming certain forms is innate in certain species, or *why* these forms are united with several other properties, if we consider the productions of nature merely as the bearers of these properties; because nothing can be inferred with regard to these inquiries from the mere observation of natural-historical properties. The only object of crystallography is, to examine the circumstances and relations under which several of these forms may appear in connection with each other, in one and the same individual, or at least in varieties of the same species. It is this consideration which renders crystallography of so much importance to Natural History, and contains the reasons why it should be treated more at large in the terminology of that science. It may be effected by purely geometrical processes, by which we obtain a certain connection among some of the forms (of which, however, it is only necessary to consider the simple ones), while between others no such connexion is manifested;—a circumstance that enables us to establish general ideas of them, so highly useful and applicable in Natural History, that, notwithstanding the introduction of mathematical consid[illegible] would remain doubtful whether it might be possib[illegible]m to arrive at any thing deserving the name of the Natural History of the mineral kingdom. These ideas are now so generally known, that we may dispense with treating them more at large in this place. We shall only observe, that they depend upon the equality of the relation between forms of the same kind, which produce series, and therefore upon these series themselves; and that it is possible to recognise and to demonstrate the internal connection between these forms, only upon the supposition of the existence of such series. It is impossible to do without these series in any system of crystallography, calculated to supply the wants of Natural History; and this in particular becomes evident, from the circumstance, that even the idea of the natural-historical species depends entirely upon the existence of these series.

The simple forms, capable of appearing in the individuals of one and the same species, or which may produce combinations with one another, are found by a particular process, called *Derivation.* This derivation, however, does not yield a number of forms undetermined in regard to the relative dimensions, one form being given; but by means of it we obtain such as are *perfectly determined* in respect to these relations. From one rhombohedron there will not result every other form of the same kind, but only those which are capable from their dimensions to form combinations,—or, which is the same thing, to appear in the individuals of one and the same natural-historical species. Crystallography, therefore, is not merely to be understood as the science that ascertains the relative position of the planes which form the limits of crystals; it must also be calculated to bring into connection the regular forms of minerals, together with their other natural-historical properties; and this is effected by means of the series arising from derivation, and the idea of the species dependent upon their existence. Their derivation at least should be the foundation of the method of providing each of the simple forms obtained with crystallographic signs,—a matter of great advantage in Natural History, for avoiding the long and tedious descriptions of minerals, which do not elucidate the subject, nor prepare us for applying calculations. The crystallographic designation should, on that account, not only denote the kind and relations of the simple forms, but also their origin, in representing the series of *forms capable of combining with each* *other;* and we should avoid such signs as, though shorter in themselves, and of equal distinctness in regard to the mathematical department, do not convey this idea of series. The theory of forms, founded upon the series, is confirmed in a remarkable manner by the physical quality of the faces which limit the forms, and of the cleavage-planes corresponding to them, and which is expressed in the former by the intensity and kind of lustre, the smoothness or roughness, the existence of striæ in certain determinate directions,—and in the latter, by their higher or lower degree of perfection, and the different facility with which they may be obtained. Although the phænomena of crystallisation are not alone sufficient to form the sole foundation of the Natural History of the mineral kingdom, as we have already observed, they yet form one of the most important departments of the properties of minerals, since, even in respect to cleavage, they are so very closely allied to the other physical qualities of natural bodies.

This connection is apparently contradicted by certain observations, which, however, will, in reality, be found rather to countenance it when viewed in a proper light. Several substances have been found frequently to assume the same form, while one and the same substance often appears under forms of two different classes not compatible with each other. The inferences generally drawn from this circumstance, were they well grounded, would indeed serve to depreciate the value of crystallography as a means of distinguishing mineral species, according to the principles of Natural History. The first of these observations, which was confined by Hauy to the forms of the tessular system, we may admit as taking place to its greatest extent: it is indifferent, whether, in this respect, we mean by *substance* the *composition* of the mineral, or the *natural-historical species.* As to the latter, the determination of the species does not solely depend upon the forms and other relations connected with it, for different species may assume *one and the same form,* although it has not yet been sufficiently demonstrated that this takes place in nature in any other species than such [illegible] ms belonging to the tessular system. But that o [illegible] substance may assume two different incompa [illegible] ue only if we consider the chemical composition of a mineral as its substance, in so far at least as our present information goes in chemistry, of which, in fact, it cannot be said, that, at some future period, something may not be discovered to explain or modify the results. The *carbonate of lime* appears in forms belonging to the rhombohedral and prismatic systems; the *sulphuret of iron* in forms of the prismatic and tessular systems; nay, a simple substance, *sulphur*, has been discovered in the forms of the prismatic and hemi-prismatic systems. But if, by the word *substance*, we mean the natural-historical species, then this is no longer true. The rhombohedral lime-haloide (calcspar) never appears in prismatic forms, nor the prismatic lime-haloide (arragonite) in rhombohedral ones; hexahedral iron-pyrites never affects prismatic forms, nor prismatic iron-pyrites such as belong to the tessular system. Nor can the incompatible forms of the varieties of sulphur be considered as occurring in the varieties of one and the same natural-historical species, even although they should exactly agree in their remaining properties (which, however, is not at present known to be the case), for the very reason that their forms are incompatible.

It may be asked, however, Whether the circumstance of the forms being incompatible is a sure criterion of the difference of two Species? The demonstration of propositions like this, in every science that is altogether dependent upon experience, must necessarily go along with experience. The laws of combination require that every simple form belonging to one and the same species, not excepting the fundamental form, should be capable of appearing in every individual of the species, whatever kind and number of forms it may already possess, or that at least it be possible to conceive this to be the case, according to certain geometrical constructions. If, therefore, two incompatible forms were to belong to one and the same species, they should appear at the same time in the same individual, which, therefore, must then be capable of containing even two different fundamental forms at once; a mode of demonstration which may be compared to the *reductio ad absurdum* in geometry. On the contrary, it is a matter demonstrated by general experience, that, in every well determined species, the simple forms belonging to its series of crystallization, appear together in the most diversified combinations in one and the same individual, but that in forming these combinations, every form is excluded which does not belong to the series. We are then entitled to ask why, among the numberless combinations in which the individuals of rhombohedral lime-haloide (calcareous spar) appear, there never occurs a form belonging to the prismatic system, nay, not even a rhombohedral form that could not be derived, according to the well-known geometrical processes, from the same fundamental form? Why, inversely, there never appear combined with the rest of the forms of the prismatic lime-haloide (arragonite), any rhombohedrons, or isosceles and scalene six-sided pyramids, regular six-sided prisms? &c. The answer to this question is, Because the rhombohedral and the prismatic lime-haloide are two different species, and because nature combines the various simple forms only *within* the limits of one and the same species, to the entire exclusion of all the rest. So long, therefore, as there is no exception to this rule, which is established upon experience, and which can be contradicted by experience alone, we possess in the fact of their being incompatible, an incontrovertible criterion of the differences between natural-historical species. Hence the inferences to which allusion has been made above, appear groundless.

The object of terminology having been thus determined, we have now to develope those general ideas and representations, which in particular might be called natural-historical ones, and of which those that regard the species are the most important. They are produced by considering the natural-historical properties, not by themselves, as in terminology, but in connection with each other, and by considering the natural productions themselves which possess them. Though these ideas have been already developed, and are generally known, (circumstances which render it sufficient to give a brief account of them in this place), yet it will be useful not to pass them over in silence, but to exhibit them in their connections, since there are some among them which apply not only to the species, but also to the genus, the order, &c.

The first is the *idea* of *Species*, which indicates that the species is [illegible]blage of homogeneous individuals, that is to say, th[illegible]tural-historical properties which may be observed [illegible]ral continues to exist, are either absolutely the same, or present gradations which form continuous series. The process of joining the series of characters together, is not only the general form of obtaining the development of this idea, but is also applicable to every particular case. The second is the *representation* of the species as a *whole*, which might, with great propriety, be called its *original* representation. The third is the *characters* of the species, by which the individuals contained in it may be distinguished from the individuals of other species. The fourth is the *general description* of the species, the object of which is to produce a distinct image of it, though we do not immediately inspect any of the varieties of the species.

That department of Natural History which embraces all these subjects, and may be more particularly said to be the philosophical part of the science, is called the *Theory of the System*, because it is the system which not only contains all those ideas and representations, but whose usefulness also can only be judged of from the quality of those ideas.

We must observe here, in the first place, that all these ideas and observations in general, refer exclusively to the natural-historical properties, because the science of mineralogy itself does not take notice of any other properties; secondly, That there is no production of nature which, as an individual body, corresponds to those ideas, the only idea which has an object corresponding to it being that of an individual. Hence in nature we find only individuals, either simple, or compound, or mixed, but we do not find species, or genera, or orders; and we must produce these ideas ourselves, in order to be able to develope Natural History as a science. In so far, a system sprung from these ideas might be called an *artificial* system, in opposition to a *natural* one. This, however, would then require to have all its general ideas *represented* by *natural bodies*, which does not take place. Individuals belonging to one species, or to one genus, &c., that is to say, which may be collected within that species, genus, &c., are the only things with which we meet in nature, and not those unities themselves. The latter would indeed be as little subject to differences of opinion or to dispute as the individual itself, if they were to be found in nature, or existed as natural productions. Hence there is no such thing as a *System* *of Nature*, or a Natural System, in the above acceptation of the phrase, because nature produces only *bodies*, and not *ideas*; and if we yet intend to make application of the expression in question, it must be in another signification, to be explained afterwards.

From the preceding considerations, it appears, that the idea of the species also, as well as every thing that refers to this idea, must be founded exclusively on the natural-historical properties, and must not contain any characteristic marks that are not natural-historical properties. We may suppose for the present, that these properties have been demonstrated to be sufficiently applicable and secure for the purpose. Whenever we introduce a chemical property, or in general any which is not a natural-historical one, we cease to be consistent, because we transgress the limits of Natural History itself. In fact, it is only pureness of principle in producing the natural-history species, that can render this species the foundation of all other sciences which treat of mineral productions, and it ceases to be useful for this purpose whenever we permit the results of these sciences to enter into the determination of the species. If, in Chemistry, we wish to refer the results of analysis to the mineral kingdom, we must compare them with the natural-historical species, without regard to any other properties, and for this end we must employ a sufficient number of *correctly determined* varieties, which, in particular, should be simple, and not intermixed with foreign substances. The results obtained by this kind of comparison with the natural-historical species, will afford the idea of a chemical species. It is sufficiently demonstrated by experience, that the different varieties of one and the same species often do not exactly agree in their mixture; and this remarkable phenomenon has given rise to many ingenious hypotheses, of which the idea of isomorphous bodies is the most interesting. It is important to observe, in respect to this subject, that these substances may be exchanged for one another in the mixture of a certain species, without having any influence on the natural-history species; their difference does not produce the slightest alteration in the for [illegible] the other natural-historical properties, particularly [illegible] nd specific gravity. If this be the case, the [illegible] mical sense, individuals differing only in their isomorphous constituents, must necessarily be considered as belonging to one and the same species, because these isomorphous substances are often but partially exchanged, and not in their whole quantity, so that a composition of both in various proportions, often takes the place of the one or the other. There is thus produced a kind of *chemical* transition, which renders it necessary to collect all those varieties within one and the same species, if we wish to avoid what would result from assuming too many of them, the entire destruction of the idea of the chemical species.

The species is the *lowest* among the systematic ideas in Natural History: For, if we proceed from the identical individuals, and unite them with whatever may be done so according to the series of characters, among which those of the regular forms are the most important, because they impart security to the employment of the rest of the series; then we immediately arrive at the idea of the assemblage of those homogeneous individuals which produce the species of Natural History. A farther distribution of the varieties into *subspecies* or *kinds* is reprehensible, because it is without the slightest advantage in a scientific point of view; impedes the easy survey of the species; and renders the nomenclature difficult or inconsistent. The species in Natural History, although the lowest, is therefore the *foundation of all the higher ideas*, in the same way as it is the formation of all those sciences different from Mineralogy, which refer to the productions of the mineral kingdom.

After the idea of the species, that of the *Genus* comes next to be considered. If, in Natural History, we have in view to proceed with consistency, the determination of this idea must be entirely dependent upon natural-historical principles. This being the case, it is evident what opinion we ought to form of such systems as have their species determined according to principles of Natural History, and their genus according to those of Chemistry. It would even seem that this want of consistency has been long ago understood, but that the difficulties attending its removal have appeared too formidable to be overcome. Yet this want of consistency is the greatest evil in every science. If it were impossible to find a principle, according to which the determination of the genus might be conducted, we should have no genera in the mineral kingdom; that is to say, the idea of the natural-historical genus would not be applicable to this kingdom.

The erroneous ideas that have prevailed in regard to the division of genus in Mineralogy, and partly also in Zoology and Botany, have been the cause that this was considered to be the case with regard to the first of these sciences, from reasons similar to those which rendered the existence of the species, and even of the individual, a matter of dispute. The genus of Natural History is nothing more nor less than the *similarity of several species*, which is much greater among some of them than among others. Vegetable and animal species, which resemble each other to such an extent, are accounted as species belonging to the same genus, and the determination of the genus does not depend upon any other consideration. Upon the same foundation, also, must it be grounded in the mineral kingdom, because Mineralogy, inasmuch as it is a part of Natural History equally with Zoology and Botany, must proceed upon the same principles with them.

So many species have already been discovered in the mineral kingdom, that their existence, or the applicability of the idea of *genus* in Mineralogy, can no longer be disputed. They are not, perhaps, all determined with perfect exactness; for this depends upon experience, which can at no time be said to be entirely exhausted; nor can this subject be more particularly considered in the present place, as we are here exclusively confined to the general development of the principles of Natural History, and their application to nature. But it is necessary to advert to another point of view from which the determination of the genus may be considered, because, if the objections dependent upon it were founded, this determination would, in fact, be annihilated. The idea of that kind of resemblance which may be called the *natural-historical* one, is said to be vague and undetermined; so that we cannot indicate upon what it depends. It is subject to a latitude of intensity, and is therefore expressed in different degrees; and, what is worst of all, it does not yield a constant rule, according to which some one or other individual might, in every cas [illegible] rred to a certain genus, or excluded from it. These ob [illegible] proceed to remove.

[illegible] *be continued.)*

Art. III.—*A Description of an Improvement in Bramah's Hydro-mechanical Press, with its application to Oil Mills.* By John Tredgold, Esq., Civil Engineer, and Honorary Member of the Institution of Civil Engineers, London. Communicated by the Author.

The powerful instrument called *Bramah's Press* is so well known, that we need not enter into a particular description of its construction. Next to the steam-engine, it has proved the most generally useful mechanical invention of modern times. It is applied, and is applicable, in all cases where intense pressure or great power is required. In our manufactories it is used for discharging colours, for pressing paper, gunpowder, &c. for packing cotton and other light goods, for expressing oils; and, in bleaching, for expressing water instead of wringing. The press is also used for drawing up piles, for rooting up trees, and for cranes for loading and unloading goods.

But, valuable as this instrument is, it has an imperfection when applied in the ordinary manner to certain purposes, such, for example, as packing cotton, discharging dyes, and expressing oils. The imperfection consists in the great variation in the power necessary to work the press at different periods of the operation, in consequence of the variable resistance of the materials under pressure at the different states of compression; which not only causes loss of time, but also, when the pumps are worked by an invariable power (as they must be when driven by inanimate power), renders the stress on the first mover irregular.

Several methods had been tried to remedy this inconvenience, but none of them succeeded in doing more than diminishing the variations in a small degree; but the invention we are now about to describe effects the purpose, and by a contrivance so simple, ingenious, and beautiful, that we are assured our mechanical readers will be interested by its description.

The effect in Bramah's press is produced by pumping a certain quantity of water into the press cylinder at each stroke of the pump; and if, with an invariable power, only one pump be employed, the quantity injected at one stroke must not be greater than can be forced in when the press is exerting its greatest pressure. Hence, in such a case as expressing oil from seeds, where the resistance in the first part of the operation is small, and increases till the compression is considered to be sufficient, the machinery must be adapted for working the pumps when at the maximum pressure, and, consequently, there must be a great excess of power in every other part of the operation.

In any hydro-mechanical press the power is proportional to the quantity of water injected, at a stroke of the pump, multiplied into the resistance; therefore, when the resistance is small, the quantity of water injected at a stroke should be increased, in order that the power necessary to work the press may be as uniform as possible, and this is the object of the patent we are about to describe *.

The machinery is applied to an oil-press (See Plate II. fig. 1.), of which M is the press-cylinder, and NN′ the bags containing the seeds; one part of the drawing shewing the exterior, and the other a section of the press boxes which contain the seed bags. LL′ are the tubes which convey the water injected by the pumps to the press-cylinder M.

I is the cistern for supplying the pumps with water, and it supports the pumps and the machinery for working them by means of the pillars HH′.

The power which works the pumps is applied to the shaft E′, and is regulated by a fly-wheel; and the motion is communicated to the other shaft E by the toothed wheels F′F. The two pump-pistons CC′ are worked by the cranks DD′, on the ends of the shafts EE′; and the cranks are made to adjust by set screws, so as to limit the length of the stroke to any required quantity within the limits of their action. The cranks act on the pump-pistons by connecting rods and slings in the usual manner.

The pump-cylinders AA′ are connected by the copper tube BB′, which is again connected to the junction-piece K by a single tube. The junction-piece K contains the stop, forcing, and discharge valves, and is connected to the tubes LL′, which convey the water injected by the pumps to the press-cylinder.

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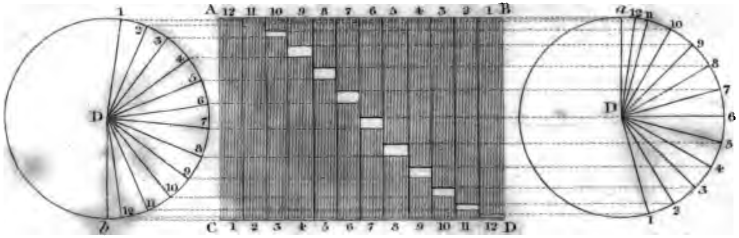
* The [illegible] improved method of working the press, was made by Mr Spiller, [illegible] tent was lately obtained by him in conjunction with Messrs Bra[illegible]

HYDRO-MECHANICAL PRESS

Fig. 3.



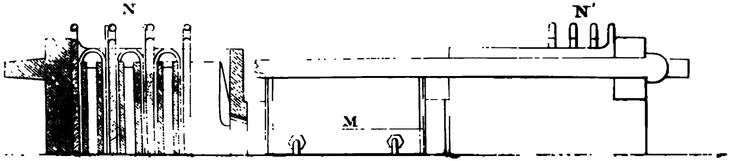
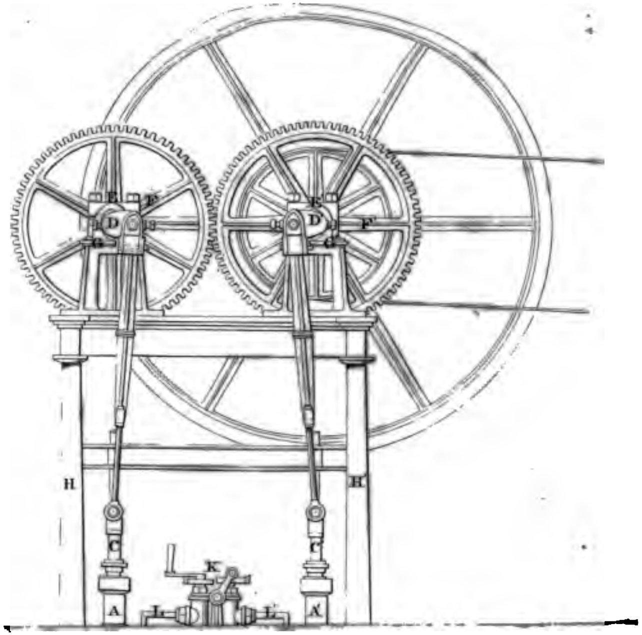
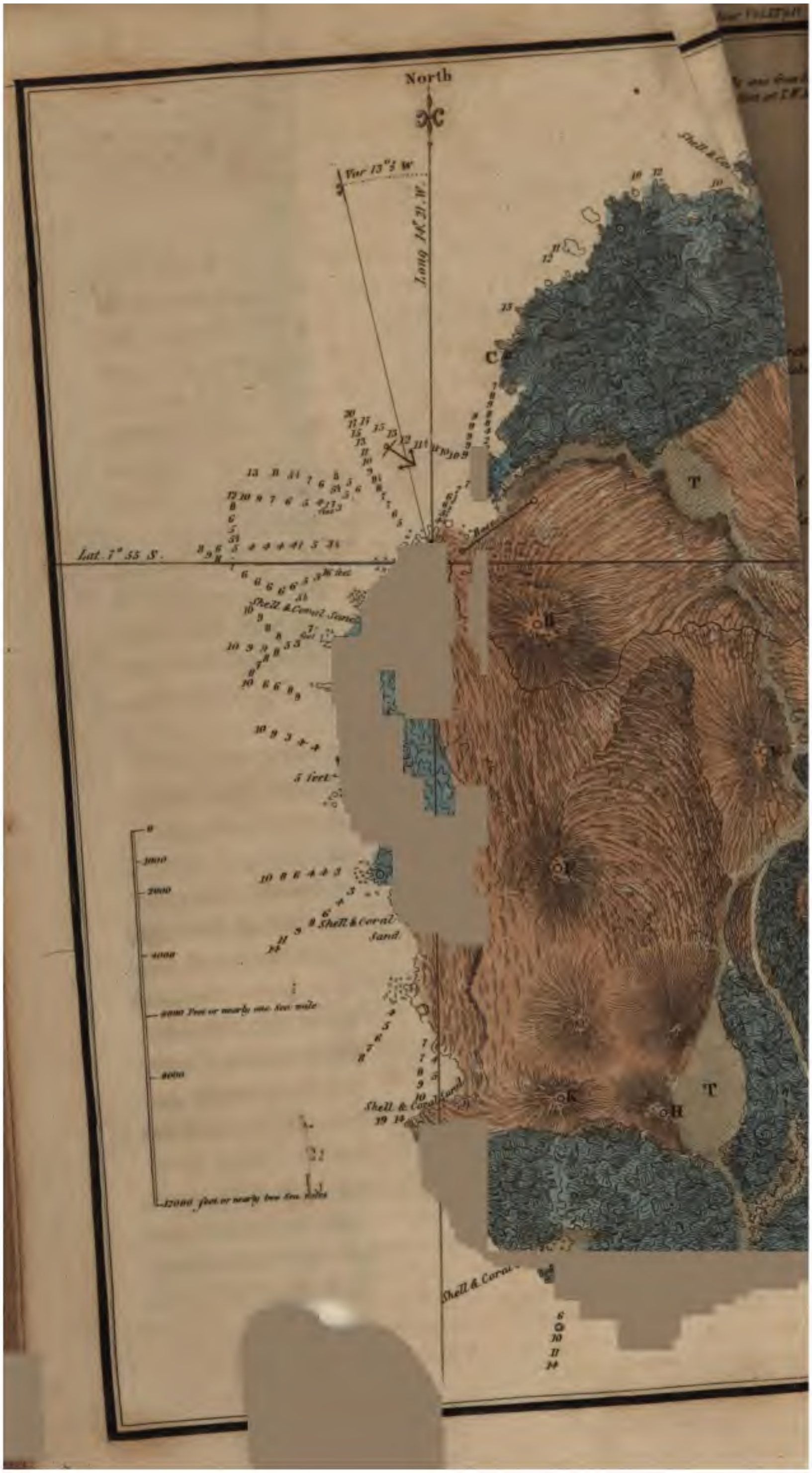


Fig. 7.




North
Var 13°5' W
Long 14°21' W.
Lat. 7° 55' S.
C
T
B
F
K
H
Shell & Coral Sand
5 feet
Shell & Coral Sand
Shell & Coral Sand
Shell & Coral
0
1000
2000
4000
6000 Feet or nearly one Sea mile
8000
12000 feet or nearly two Sea miles

The whole island has a most forbidding and rugged aspect. Its highest mountain, named Green Mountain Peak, is 2818 feet above the level of the sea The largest portion of the mountain is 2000 feet above the sea; and at this height there is a space of comparatively level ground, in which the principal *garden* in the island is situated. From the top of the Peak down to about this level, or a little lower, the surface, excepting where it is precipitous, is covered with a coat of soil, which is nowhere deep, and having under it masses of pumice and lava. The precipices around this height, are, in many instances, formed of slaggy lava; and, in the lava, are veins filled with opal, containing imbedded fragments of vesicular and slaggy lava. In other parts, there are rocks of a felspar or trachyte porphyry. Among the many ridges shooting from the Green Mountain (M of the chart), one of the most remarkable is that composed of black and dark-green perfectly formed obsidian, which, in some places, is disposed in balls and globular concretions, like that found in Kamtschatka; and, in others, in large globular concretions, like those of basalt and greenstone. Associated with it there are grey varieties of pearl-stone *. This vitreous mineral is there associated with various porphyries, apparently trachytic; and, in some places, green pitchstone, with imbedded sphærulite and common pumice and pumice-conglomerate, occur. Not far from the obsidian ridge, there is a remarkable hill, named by the sailors *The Devil's Riding-School*, marked in the chart P. It is about 700 feet above the level of the sea, and between 400 and 500 feet above the surface of the surrounding base. It has a circular hollow on the top, which probably was formerly much deeper than at present, it being now filled up to within 30 feet of the edge of the crater. This hill, as far as can be made out from the specimens brought home, appears to be composed of trachytic rocks. In some varieties, the basis is like claystone, and contains imbedded portions of slaggy lava; in others, the basis is of felspar, with imbedded crystals of glassy felspar, and fragments of slaggy lava; and the trachyte porphyry sometimes contains, in its cavities, crystals of Vesuvian. Many of the rocks are in an earthy state, owing to the action of the weather; and occasionally they are observed decaying in globular and concentric lamellar concretions. The upper and middle parts of the hill, marked B in the chart, are composed of vesicular, spumous, and corded lava. Some of the vesicular varieties much resemble the millstone lava of Andernach. The lower part of the hill consists of rocks of a different description, which form, as it were, a foundation on which the vesicular and corded lavas rest. On the SW. side, the rocks are trachyte-porphyry, occasionally including fragments of slaggy lava. On the NE. side is a bluish clinkstone-lava, with numerous imbedded felspar crystals.

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* Specimens of vesicular iron-ore were found in a trachyte ridge not far from the obsidian ; [illegible] tals of specular iron-ore, like that of the Island of Stromboli.

It thus appears, that the Green Mountain, and the hill P, are composed of trachyte, and its congenerous rocks; while B consists of vesicular and slaggy lava, resting upon trachyte. All those parts of the island coloured in the chart *reddish-brown*, are of the same description. The rugged parts of the island, all of which are coloured *bluish-black* in the chart, are composed of a greyish-black lava, slightly vesicular, and containing few crystals of glassy felspar. This lava presents a frightfully rugged surface, which forms irregular eminences, varying in height from 20 to 50, and even 100 feet.

In the bays, and on such parts of the coast as are not precipitous, the beach is formed of a sand of comminuted shells, with fragments of echini and of corals. In some places near to the sea, the fragments of shells are conglutinated together by a calcareous cement, and form a pretty solid mass. The solidity of the mass diminishes as the distance from the sea increases. A turtle's nest, with eggs, was observed imbedded in this conglomerate. The rocks which rise through these calcareous beaches, and which are so near to the sea as to be washed by its spray, are incrusted with a calc-sinter and calc-tuff, formed by the action of the weather on the calcareous matter of the shells and corals.

Lastly, it may be mentioned, that runs of a sand, composed of the materials of the rocks, occur in different parts of the island, and that these are pointed out in the chart by the *pale yellow* colour.

Baron Von Buch divides volcanic islands into three classes, which he characterises in the following manner:

1. *Basaltic Islands.* Composed of strata of basaltic rocks, in which there is generally a crater of elevation (Erhebungs crater.)

2. *Volcanoes.* Isolated; very elevated peaks, and domes of trachyte, and generally with a great crater on the summit.

3. *Erupted Islands.* These have been formed by single eruptions, and scarcely ever occur without basaltic islands.

The Island of Ascension is, by Von Buch, referred to the third division; but it now appears, from the facts stated above, that this island belongs not to the third alone, but rather conjoins in it the characters of the second and third classes*.

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* Professor Jameson had the goodness to examine the different rocks enumerated above.

ART. VII.—*A Catalogue, in Right Ascension, of 46 principal Stars, deduced from Observations made at the Observatory of Trinity College, Dublin, in the years 1823 and 1824.* By the Rev. Dr BRINKLEY. Communicated by the Author.

Stars.	Æ 1825.	Ann. Var. 1824.	Sec. Var.
	h ′ ″	″	″
γ Pegasi,	0 4 13,91	+ 3,077	+ 0,010
α Cassiopeæ,	0 30 37,85	3,333	0,051
Polaris,	0 58 17,10	15,000	
α Arietis,	1 57 19,52	3,354	0,020
α Ceti, -	2 53 8,26	3,120	0,010
α Persei,	3 11 52,30	4,221	0,049
Aldebaran,	4 25 53,12	3,427	0,011
Capella,	5 3 46,31	4,411	0,019
Rigel,	5 6 7,75	2,877	0,004
β Tauri,	5 15 13,97	3,781	0,009
α Orionis,	5 45 41,83	3,243	+ 0,003
Sirius,	6 37 25,96	2,643	0,000
Castor,	7 23 25,07	3,847	— 0,012
Procyon,	7 30 8,08	3,146	0,004
Pollux,	7 34 35,56	3,684	0,012
α Hydræ,	9 13 59,15	2,948	0,001
Regulus,	9 59 2,41	3,204	0,010
α Ursæ majr.	10 52 50,65	3,801	0,086
β L[illegible]	11 40 7,47	3,065	0,008
γ [illegible]	11 44 34,79	3,208	0,046
ε [illegible]	12 46 17,78	2,670	— 0,029
[illegible]	13 15 58,93	3,143	+ 0,011
η [illegible]	13 40 37,95	2,377	— 0,011

Stars.	Æ 1824.	Ann. Var. 1824.	Sec. Var.
	h ′ ″	″	″
Arcturus,	14 7 40,77	2,730	+ 0,001
α^1 Libræ,	14 41 1,20	3,297	0,016
α^2	14 41 12,61	+ 3,300	0,016
β Ursæ minor.	14 51 18,91	— 0,301	0,111
α Cor. bor.	15 27 16,68	+ 2,534	0,002
α Serpentis,	15 35 39,09	2,947	0,006
Antares,	16 18 41,33	3,658	0,015
α Herculis,	17 6 40,17	2,729	0,004
α Ophiuchi,	17 26 48,74	2,775	0,003
γ Draconis,	17 52 32,61	1,390	0,004
α Lyræ,	18 31 0,73	2,028	+ 0,002
γ Aquilæ,	19 37 56,22	2,853	— 0,001
α	19 42 14,49	2,927	0,001
β	19 46 42,82	2,948	0,001
α^1 Capricorni,	20 7 56,33	3,331	0,008
α^2	20 8 20,14	3,335	— 0,008
α Cygni,	20 35 27,90	2,038	+ 0,002
α Cephei,	21 14 23,68	1,440	— 0,006
β	21 26 21,92	0,812	0,032
α Aquarii,	21 56 47,45	3,083	0,004
Fomalhaut,	22 47 57,63	3,338	— 0,022
α Pegasi,	22 56 2,84	2,979	+ 0,005
α Andromedæ,	23 59 21,43	+ 3,076	+ 0,018

The above Right Ascensions are, in their mean quantity, about 0″,2 less than those of M. Bessel; and about 0″,3 less than those of Mr Pond. The *annual variation* is determined by comparing this catalogue with Bradley's catalogue in the *Fundamenta Astronomiæ.*

Mean error of the catalogue in Æ *in space*, by observations of the Sun in Spring and Autumn, with the 8-feet astronomical circle:

	Days obs.	
Autumn 1822, Spring 1823,	10 11	$+ 0'',40 + 0'',04\, dL - 0'',06\, dr - 0'',07\, dO$
Autumn 1823, Spring 1824,	18 18	$- 0,62 - 0,11\, dL + 0,10\, dr + 0,14\, dO$
Autumn 1824, Spring 1825,	16 18	$+ 0,34 + 0,04\, dL + 0,10\, dr - 0,16\, dO$

Mean error of Catalogue $= + 0,04 - 0,01\, dL + 0,05\, dr - 0,03\, dO$

where dL = error in latitude. dr = error in constant of refraction. dO = error in obliquity of ecliptic.

The small coefficient of dr shows that the error arising from the errors of division must be absolutely insensible.

ART. VIII.—*Account of a Bridge of Suspension made of Hide Ropes in Chili.* By Captain BASIL HALL, F.R.S. Communicated by the Author.

OVER the river Maypo, at no great distance from the city of Santiago, the capital of Chili, there is thrown a bridge of a curious construction. It consists of a roadway, four feet broad, of planks laid crosswise, with their ends resting on straight ropes, made of twisted thongs of undressed bullocks' hides, which are suspended by means of short vertical lines, about as thick as the little finger, to a set of stout ropes drawn across the valley from bank to bank. These strong sustaining cords are six in number, three at each side of the bridge, and hang in flat curves, one above another. They are firmly secured to the rock, at the top of the bank on one side, at the height of twenty or thirty feet above the bed of the stream; but on the opposite side, where the bank is low, they are made to pass over a high frame-work of strong timbers, the nature of which will be more readily understood by a reference to Plate IV., than by any description. The consequence of the different elevation of the two banks is, that the bridge has a very considerable slope,—a circumstance which adds to its picturesque effect, while it takes little from its utility, as it is not intended for wheel-carriages.

The clear space, from the frame-work on one side to the face of the rock on the other, is 123 feet. The materials are very elastic, and the bridge waves up and down, and from side to side, in so alarming a manner, that a stranger is glad to dismount and lead his horse across, or, as we preferred doing, at the recommendation of our guides, drive it before him.

It will be apparent, at the first glance at the Plate, that there is a remarkable similarity between this hide-bridge and those of iron with which we are now so familiar in this country. A more careful inspection will only show, that the resemblance extends even to minute particulars, one of which is very striking, —I mean the manner in which the weight of the road is distributed over the suspending or curved ropes. It will be observed, that the first of the small vertical lines is attached to the upper rope [illegible] is fastened to the middle one, and the last to the lowe [illegible]is series is repeated along the whole length, exactly [illegible]e bridge of suspension across the Tweed, and in t[illegible]haven, and in other similar structures.

I was informed on the spot, that these South American bridges were found, exactly as they now exist, by the Spaniards, when they first occupied the country three centuries ago; and it is quite as certain, that nothing was known of this principle, as applied to iron, till within these few years.

I have not heard whether Captain Brown, the well-known inventor of the Chain-Cable, and who first introduced the iron-bridge of suspension, claims it as an original invention. His merit, however, is not, as I conceive, in the smallest degree lessened, by supposing him to have seen or heard of these hide-bridges of South America; for it is quite as praiseworthy an exercise of genius and talents to observe and turn to account such analogies as these, as it is to invent what is altogether new. Indeed, this is one of the broadest distinctions, by which the mere visionary theorist is separated from the useful, practical adapter of known and tried principles to the business of life.

It is, however, a curious subject of scientific history, to trace the progress of such inventions and adaptations, from their rudest to their most perfect state; and I shall be very happy if this notice shall have the effect of inducing the ingenious and able officer alluded to, to favour the public with such an account, not only of this invention, but also that of the chain-cable, which, as a seaman, I may be excused in describing as one of the most important applications of principles with which every person was familiar, but no one turned to account, till the sagacity and perseverance of Captain Brown taught us their use.

Addition by the Editor.

In an interesting Report by Captain Brown, "on the proposed plan of erecting a Patent Wrought-Iron Bridge of Suspension over the Thames, near Iron-Gate and Horslydown," which we hope to lay before our readers in the present or next Number of this Journal, the following remarks occur, which bear on the subject of Captain Hall's account of the Native American Bridge.

"It will not at all lessen the importance of the present proposal, if it be admitted that bridges of suspension have long existed in other countries, and it cannot be pretended by any man that a new principle has been discovered. The properties of the catenarian curve are obvious in the Indian bridge of suspension, formed of ropes or bamboo canes, and in those constructed of common chain, as well as in a variety of objects which must be familiar to every person of common observation. But those simple contrivances, which have been noticed by some writers, have no more resemblance in their construction to the bridges or piers of suspension which have been erected in Great Britain, than the rude bridges of remote ages, which consisted of logs supported on props, are to be compared to the architecture of modern times.

“ The first bridge of suspension that we hear of in this country, is the one thrown across the river Tees, in the county of Durham, the span of which does not, I think, exceed 80 feet *. It is formed of two common chains, stretched over the river, from abrupt banks, with battens laid across, and boarded, the gangway partaking of the curve of the chains.

“ Such an arrangement is evidently a bad one, inasmuch as we must ascend to the points of suspension, then descend, and rise according to the curve of the chain, which, in that which I have usually adopted, would be a pull of one foot in seven. This is hardly practicable, and my earliest attention was employed to remedy the evil. In 1814 I erected a bridge, with the road or platform perfectly horizontal, on my premisses at Mill-Wall, where it still remains. This is effected by introducing perpendicular rods through the joints of the main suspending bars, and adjusting their length to the curve above, so that they form a series of straps for the reception of a row of bars on each side, placed edgewise, and extending the whole length of the bridge, parallel to the entrance. The beams being laid across these bars, the platform or road becomes quite horizontal; or an ascent may be given from the sides to the middle, in the same plane as with the roads leading to the bridge. The span is 105 feet, and the iron-work only weighs 38 cwt. It was inspected by the late Mr Rennie and Mr Telford, who drove their carriages over it; and it has been considered by men eminent for their skill in mechanics, as a remarkable combination of strength and lightness.

“ T[illegible]ce to improvement in this new era of bridge-buildin[illegible]aced to the invention of iron-cables, which

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* An [illegible]ridge is given in pages 238 and 239 of Vol. V, of the Edinburgh[illegible]rnal.—Edit.

necessarily introduced the powerful proving machine. A knowledge of the strength of bolts and bar iron of large dimensions, was thereby obtained, which formerly was deduced from trivial experiments, leading to most erroneous calculation; and as the importance of this new branch of naval equipment developed itself, the principal iron manufacturers of England vied with each other in its improvement; and British iron is now brought to a state of perfection, that will, for general purposes, entirely supersede the use of foreign. There is also a uniformity in the strength of the improved British iron, beyond that of any other country; so that by adopting straight bolts or bars, united end to end in the direction of their length, by coupling plates and pins of proportionate strength, instead of chains, we have an increase of strength with less weight; the risk of bad workmanship is almost entirely obviated; and the subsequent proof to which every part of the work is subjected, reduces the calculation of its strength to a certainty."

These observations state distinctly the extent of Captain Brown's claims in this great work of improvement. On conversing with this active and ingenious officer, on the subject of the bridges of suspension observed in South America, and other countries, he said, that the only one which has the road on the same plane with the banks, is that here described by Captain Hall; all the others which he had heard of, having the road erected over the chains, and partaking of the curve, which, with a flexure necessary for the security of the bridge, rendered the passage very inconvenient: Further, that his observations were written before Captain Hall could have seen the bridge over the river Maypo in Chili, and with which he now, for the first time, became acquainted. He claims whatever merit may be due to the mode of construction, which is entirely new, and for which he obtained a patent seven years ago. The model of this original plan is erected in Captain Brown's premises at Mill Wall, on the river Thames, near to London, and is, as above stated, 105 feet span, and strong enough to carry loaded carriages. In 1819 he erected a bridge on the same plan over the river Leader, at Carolside in Berwickshire; and he is now constructing the iron-work of the bridge over the Thames at Hammersmith, with scarcely any variation, at least none that he considers as a deviation unconnected with the necessary arrangements of a bridge on a larger scale.

Art. IX.—*Observations for determining the Magnetic Variation, made in the Neighbourhood of Spitzbergen, by Capt. (then Lieut.) Franklin, assisted by Lieut. Beechey, Mr Back, and Mr Fyffe, in His Majesty's Ship Trent, in the year* 1818. Communicated by Captain Franklin.

Time.		Lat. North.	Long. East.	Variation per Azimuth.				Bearings of Land.	Remarks.
Date.	*Hour.*			*Walker's Meridion. Compass.*	*Kater's Compass, No. 1.*	*Kater's Compass, No. 2.*	*Mean.*		
	7 P. M.	79° 50′	11° 45′	21° 14′	... ° ′	26° 28′	23° 41′	... ...	Taken on the ice at a distance from the ship.
	6½ ...	79 57	11 21	21 49	24 19	24 5	24 12	Cloven Cliff, S. 38° W. distant 10 miles.	Taken on the ice at a distance from the ship. Walker's compass traversed so sluggishly, and the observations made with it to-day varied so much, that the mean is struck from Kater's two compasses.
... 21.	6¼ A. M.	79 56	11 32	23 37	...	...	23 37	Cloven Cliff, S. 81° W. Vogel Sang, S. 78° W.	On ice at a distance from the ship.
... ...	6½ P. M.	79 56½	11 32	24 08	...	25 08	24 30	Cloven Cliff S. 87° W. Grey Hook, S. 48° W.	Ditto.
							24 03	... ...	Mean of two preceding observ.
... 22.	7 ...	79 57	11 15	...	...	24 17	...	Cloven Cliff, W. 7° N. Red Point S. 23° E.	On ice at a distance from the ship.
July 2.	8¾ ...	79 48	10 15	...	24 11	24 12	24 12	... ...	At the observatory in Fair Haven on a small island.
... 10.	7½ ...	80 19	10 31	23 33	26 33	26 2	26 18	In sight of Amsterdam Isl.	On ice, &c.
... 19.	6½ ...	80 24	11 14	...	24 0	24 5	24 2½	Ditto.	On ice, &c.
... 21.	7 ...	80 12	13 2	20 56	24 27	24 47	24 37	Grey Hook, S. 37° E. Red Point, S. 7° E.	On ice, &c. Means from Kater's compass.
... 24.	6¼ ...	80 18	11 38	...	23 56 25 02	...	24 29	... ...	On ice, &c.
Aug. 8.	8 A. M.	79 39	10 0	...	24 50	...	24 31	... ...	On SW. point of Danes' Isle, at the observatory.
... 12.	12 P. M.				...	24 21			
... 27.	6¼ A. M.				...	24 19			
[illegible]	7					24 58			

ART. X.—1. *On the Unequal Distribution of Caloric in Voltaic Action.* 2. *On the Temperature of the Skin of the Dormouse.* 3. *On the Temperature of the Egg of the Hen, in relation to its Physiology.* By JOHN MURRAY, F. S. A. F.L.S. & M.W.S. Communicated by the Author.

1. *On the Unequal Distribution of Caloric in Voltaic Action.*

THE following experiments may, I hope, be found interesting, and eventually throw light on the more obscure features of galvanic action, while the thermo-electric phenomena of Seebeck, Dessaignes, Moll, Van Beeck, &c. may be elucidated. Even the occult meteorology of the thunder-storm may find facts amid experiments such as these are, that may ultimately conduct to a happier theory.

Four galvanic troughs were employed. They were constructed in the *triad form*, on the principles of Dr Wollaston; and the cells containing the acid were of porcelain. Each trough had 10 triads, and the plates were 4 inches square.

I used 1½ oz. of *nitrous acid* for each compartment, and filled up with water. About 6 inches of platinum wire, $\frac{1}{50}$th inch diameter, were ignited, &c.

Air 66° Fahr.; water in room 64°. 2d September 1823.

First Trough.

Zinc end—positive.

Cell 1.	Temp.	99° F.
2.	-	102
3.	-	104
4.	-	106
5.	-	108
6.	-	110
7.	-	111
8.	-	112
9.	-	110
10.	-	108

Diff. between minimum and maximum, 13° F.

Second Trough.

Cell 1.	Temp.	99° F.
2.	-	100
3.	-	102
4.	-	102
5.	-	102
6.	-	99
7.	-	97
8.	-	95
9.	-	93
10.	-	91

Diff. between minimum and maximum, 11° F.

Third Trough.

Cell 1.	Temp.	101° F.
2.	-	104
3.	-	106
4.	-	108
5.	-	108
6.	-	108
7.	-	106
8.	-	105
9.	-	103
10.	-	101

Diff. between minimum and maximum, 7° F.

Fourth Trough.

Cell 1.	Temp.	100° F.
2.	-	102
3.	-	103
4.	-	104
5.	-	103
6.	-	101
7.	-	100
8.	-	100
9.	-	99
10.	-	98

Diff. between minimum and maximum, 6° F.

Copper or negative termination or pole.

In the preceding experiments, the plates of zinc having been much corroded, might be expected to affect the results, and render them somewhat equivocal. The phenomena, however, seemed to indicate a gradual *declension* of temperature from the positive to the negative pole. It is curious, too, that the alternate troughs singularly coincide. It was also evident, that there obtained a *maximum* near the central region of the individual trough, with a gradual declension in each, pointing in the direction of the negative pole. These facts prove, that an unequal distribution of temperature is associated with the production of galvanic phenomena.

The plates *were renewed* in the experiments which succeed. On the 18th September 1823, temperature of water 62°, the same strength of acid was employed. From 14 to 15 inches of platinum-wire, $\frac{1}{100}$th of an inch diameter, were made *white hot.* In the first series, the temperature was taken *before the plates were removed*, and when the action had been reduced to the ignition of a few inches of the wire. It commences with the copper or negative cell.

	1st Trough.		*2d Trough.*		
(Copper end.)—	Last cell,	101° F.	Last cell,	125° F.	
	Middle,	106	Middle,	140	
	First,	112	First,	135	
	3d Trough.		*4th Trough.*		
	Last cell,	138° F.	Last cell,	136° F.	
	Middle,	141	Middle,	142	
	First,	138	First,	142	*(Zinc or positive end.)*

It appears from the foregoing, that the *minimum* of temperature in the aggregate troughs is at the copper or *negative* limit, and the *maximum* at the *positive* or zinc termination. In three of these troughs the maximum of the individual one is still maintained toward the centre.

When the plates were removed, the following was the exhibition of temperature.

	1st Trough.	*2d Trough.*	*3d Trough.*	*4th Trough.*	
(Copper plate.)	101° F.	123° F.	128° F.	128° F.	
	106	125	129	129	
	109	127	130	131	
	110	129	131	133	
	111	131	132	134	
	[illegible]	133	133	134	
	[illegible]	134	133	133	
	[illegible]	133	131	133	
	[illegible]	131	130	132	
	[illegible]	129	129	132	*(Zinc plate.)*

In the preceding the grade of increment from the negative to the positive pole is remarkably uniform. Towards the centre of the individual trough, the maximum still obtains. The last cell at the copper pole is decidedly the *minimum*, being only 101° Fahr., while that of the zinc pole is 132° Fahr., a difference of 31° Fahr.

The experiments which succeed were made on the 6th October 1823, air 63°, diluted acid in cells before immersion of the plate 64°.5 Fahr.

So soon as the plates were plunged into the cells,

Zinc end (positive).	1st Trough	69°,	Centre 66°,	End 67°	
	2d ——	70,	—— 68,	— 75	
	3d ——	80,	—— 75,	— 75	
	4th ——	94,	—— 86,	— 84	*Copper (negative.)*

Before the action is fairly determined, the above experiments prove the negative end to sustain the maximum of temperature, being 84°, while the positive end is 69°.

Before removal of the plates, when the acid had become weak,

Zinc (positive).	1st Trough	126°,	Centre 125°,	End 124°	
	2d ——	126,	—— 130,	— 126	
	3d ——	124,	—— 128,	— 130	
	4th ——	124,	—— 122,	— 120	*Copper (negative.)*

Here, as in former experiments, the maximum is at the zinc, the uniformity already named being remarkably sustained.

After removal of the plates the indications of temperature were as follows.

1st Trough. (Zinc or positive.)	*2d Trough.*	*3d Trough.*	*4th Trough.*
Cell 1, 122° F.	Cell 1, 122° F.	Cell 1, 121° F.	Cell 1, 121° F.
2, 124	2, 124	2, 122	2, 122
3, 126	3, 125	3, 124	3, 122
4, 126	4, 126	4, 125	4, 122
5, 126	5, 126	5, 125	5, 121
6, 125	6, 128	6, 125	6, 119
7, 124	7, 127	7, 125	7, 116
8, 123	8, 126	8, 125	8, 116
9, 120	9, 124	9, 129	9, 116
10, 120	10, 122	10, 126	10, 116
			Copper or negative.

Several queries seem naturally to spring from these facts. Does the excited electricity thus modify the distribution of heat? or, Does the chemical action of the acid on metals of different conducting powers produce the unequal balance; and is electricity the consequence of this unequal distribution?

The action of electricity may either give rise to an unequal distribution of temperature, or an unequalized temperature give birth to electrical phenomena; which disturbed balance of temperature it is the province of electricity to restore, and hence the thunder-storm is commissioned to determine the equalization and distribution. This last view of it seems to me most probable, and affords a satisfactory view of the beneficent arrangements of Providence.

In corroboration of this conclusion, it may be interesting to state the remarkable change of temperatures which I ascertained to take place during my journey last summer from Basle on the Rhine to Paris.

On the 10th September, at a quarter past 6 P. M. near to Montmiral, the thermometer indicated 79° Fahr.; and the horizontality of the clouds announced the distant thunder-storm. In ten minutes the instrument rose to 84°.5; and at half-past 6 stood at 74°. Distant lightning. Thermometer subsequently ascended to 90° Fahr.; and about 7 o'clock had fallen to 73° Fahr. It then rose to 78° F.

PAISLEY, 11*th Nov.* 1825.

2. *On the Temperature of the Skin of the Dormouse.*

The strange repose of *toads*, *frogs*, and *lizards*, in the solid and almost hermetically sealed rock, is a phenomenon important to the geologist, and calculated to excite the liveliest interest in the physiologist. I have paid some attention to the question of *torpidity in animals*, and ever delight rather to register *facts* (especially where the question is hypothetical), than to speculate in the regions of theory.

In the beginning of last year, I received two dormice from a friend in Derbyshire, and commenced a series of experiments on the temperature developed by the skin. One of these I accidentally lost, it having escaped from confinement; and I was shortly necessitated, from various avocations, to resign the prosecution of my researches with the other. The following is a note of the temperature as recorded:

31st Jan[illegible] 1824, Chesterfield, Derbyshire. At 7h and 25′ P. M. air of room 48° F[illegible]ature of the dormice under the breast 103° Fahr.—I soon after [illegible] prisoners.

At Hul[illegible]th February, at 8 and 30′ P. M., air 51° Fahr., temperature u[illegible]′ Fahr. *The animal semitorpid.*

Feb. 15. At 1^{h} 15′ P. M., air 46°, under breast 104°
— At 8 30 —— — 47°.5, —— —— 69° *semitorpid.*
— At 3 30 —— — 52°, —— —— 102°.5
19. At 2 —— — 56°, —— —— 99°
21. At 10 30 —— — 54°.5, —— —— 102°
22. At 12 30 —— — 57°, —— —— 97°

On the 14th and 15th February, the dormouse was roused from its apparent death by heat cautiously applied.

The box which contained the dormice had a partition. One compartment contained fresh moss, well dried, in which the animals reposed *during day*, having formed for themselves a somewhat elliptical nidus. Two openings conducted into the *outer court*, where the dormice had their food prepared for them, consisting of wheaten bread (sometimes softened with water), and a basin of milk. Great attention and care were bestowed on them, and the food daily supplied.

Though their cage was frequently in darkness during the day, the *night season* was the exclusive period in which they took food. One of them had a singular expedient, when the liquid was too low in the basin. It *dipped its brushy tail* (somewhat resembling that of a fox) *into the dish*, and carried the milk in this manner to the mouth. When the dormice are torpid, they may be thrown up like a ball, &c. without any indication of motion, or change of state.

9th Nov. 1825.

3. *On the Temperature of the Egg of the Hen, in relation to its Physiology.*

There has long existed a curious and very peculiar test for discovering the relative freshness of the egg. I particularly advert to that of the hen, but presume the same discriminating test would be generally applicable.

The tip of the tongue, when brought in contact with the several ends of the egg, experiences a peculiar sensation, caused by *a difference in temperature*, the *great* end being sensibly warmer. The following experiments, made with a very delicate thermometer, and effected with considerable care, determine that this peculiarity is not imaginary; and though the slight difference may, at first sight, appear to militate against the conclusion, let it not be forgotten, that the tongue, thus applied, is a very sensible and delicate test. I feel persuaded, that, in this way, I can readily discriminate *between the colours*, in relation to *differently coloured petals in flowers;* and, moreover, it was thus that Mr John Gough of Kendal, though blind, determined the temperature evolved in the dilatation and contraction of caoutchouc, for which see his paper in the Transactions of the Manchester Philosophical Society.

The cause of this unequal distribution may be clearly traced to the *cicatricula*, from which the caloric seems to radiate. When we puncture the shell, the cicatricula may be discovered floating in the *albumen*, on the acclivity of the *vitellus*, and near the summit of the globe toward the great end.

In the following experiments, the projecting minute ball of the thermometer was very cautiously and carefully immersed into the albumen, when the shell at either end was broken to allow its introduction. The external atmosphere was at same time registered.

External air, 52° F.
Small end of egg, 58°.5, ¾ths of an inch deep, 60°
Great end, 59°, ——————— 60

Small end, - 56°.5, deep, 58°
Great end, - 58°, —— 58°.5

Air, 63°5
Vitellus, 64°.5 } In another, { 66°.5
Albumen, 64°.5 } { 66°.5

In these the thermometer was deeply immersed, and it is evident that the difference does not arise from the vitellus or albumen, or any specific phenomenon connected with them individually.

Small end, 58°5, immersed, 61°.5 +
Great end, 60°, ——— 61°.5 +

From side to centre, 65°
In another, - 64°

Air, 52° +
Small end, 57°.75, immersed, 58°.5
Great end, 58°.5, ——— 58°.75

Air, 54°
Small end, 58°.5, immersed, 59°.5
Great end, 59°.5, ——— 59°.75

Air, 52°
Small end, 58°.5, immersed, 59°.25
Great end, 59° + ——— 60°

Small end, 85° F.
[illegible] 1 in. 87°5
[illegible] 88° F.
[illegible]n. 93°
[illegible]f the cicatrice, 91°.5
[illegible] - 73°
[illegible]nk to 55°.5 by the evaporation of the albumen.

It will be seen, that, throughout these experiments, the egg maintained a temperature superior to that of the external medium, even, in the latter instances, though that medium was considerably exalted.

10*th Nov.* 1825.

Art. XI.—*Remarks on Mr Daniell's Hypothesis of the Radiation of Heat in the Atmosphere.* By Mr Foggo *junior.* Communicated by the Author.

THE few remarks I am to offer on this hypothesis, were originally intended to have been inserted in a note to the Meteorological Register published in this Journal. It has been thought proper to give them a separate place, chiefly with the view of exciting more attention to the subject among those who have opportunities of prosecuting such inquiries.

Before the publication of Mr Daniell's essays, solar radiation had never been treated of to any extent more than conjecture, or a few unconnected experiments. Besides the interest which it possesses as a subject of experimental research, there are several questions of the highest consequence to physiology, which depend upon our knowledge of this important agent. Some of these, which Mr Daniell himself had principally in view, are here given in his own words: " Does its influence increase with the temperature of the air from the Poles to the Equator ?" or, " Is the rapid vegetation of the Arctic Regions, during the short summer of these climates, dependent on any compensating energy of its operation ? Before I attempt to answer these questions, I shall propose another, which many will be surprised to find cannot be met with an immediate solution ; which is, the maximum degree of heat to which a plant, or the parts of a plant, are subjected, by exposure to a mid-day sun, in midsummer, in this climate ? There are, no doubt, in all plants, parts which are calculated to absorb all the radiant heat which strikes upon them ; and it is therefore desirable to know, with a reference to this subject alone, the utmost amount of temperature which radiant matter is capable of producing. My Meteorological Register includes a column for observations upon this point. They are complete from November 1820 to the end of

December 1821, and from the beginning of May 1822 to the end of August of the same year. They were made by means of a register-thermometer of large range, having its bulb covered with black wool, and placed upon a south border of garden-mould, with a full exposure to the sun. The thermometer did not rest on the earth, but was supported about an inch above it. The maximum-heat of the sun's rays during the day was thus measured, and recorded in the journal."

At the request of Mr Daniell, Captain Sabine made many observations for the same purpose, in different places within the Tropics. From a comparison of the results obtained by himself, with those of Captain Sabine, he infers, that the intensity of the sun's direct rays decreases as we approach the Equator. And extending the comparison to a few facts connected with this subject, which are recorded in the late voyages to the Arctic Regions, he considers the conclusion to be incontrovertible, That the intensity increases proportionally as the distance from the Equator. A theory is given in support of this singular proposition; but with respect to this, we have no doubt Mr Daniell himself has already discovered the oversight by which he has been misled.

A consideration of the experiments themselves will, however, afford some interest. But it must be here observed, that Mr Daniell, in his Essay, applies the term, "force of radiation," indiscriminately to phenomena essentially distinct; so that the actual power of the sun's rays is confounded with the excess of temperature indicated by a thermometer exposed to the sun, above the temperature of the air. As it is only to the former that my remarks are at present directed, instead of the table given in the essay itself, we shall extract from his journal the maximum temperatures registered by the black thermometer.

January,	60°	May,	135°	September,	120°
February,	80	June,	154	October,	104
March,	97	July,	128	November,	72
April,	110	August,	144	December,	66

Here the maximum observed is 154°. This took place on the 5th of June; on which day, the little breeze then blowing being from tl [illegible] the thermometer must have been completely screene [illegible] ıg effects. Of the tropical observations, we shall qu [illegible] ıade at Bahia and Jamaica, as they alone can enter into comparison with Mr Daniell's. A mercurial thermometer, having its bulb blackened, and covered with black wool, was fully exposed to the sun, *on grass*. The following are the results obtained at Bahia:

July 24.	114°	July 28.	95°
25.	123	29.	115
26.	124	30.	127
27.	123		

These results were obtained during a southerly wind, with frequent rain, surely not the most favourable weather for such experiments. At Jamaica, with the same thermometer, the highest temperature observed was 123°; but no remarks are given on the state of the weather. M. Gay Lussac objected to these experiments, as having been influenced by the vegetation on which the instruments reposed. As Mr Daniell rests the truth of his opinion chiefly on these experiments, we made a few trials, to ascertain the probable amount of such effects upon the results. On the 7th of July last, temp. of the air 59°, with brisk wind, we exposed a large thermometer, having its ball covered with black wool, to the direct rays of the sun, but not sheltered from the wind. In 10′ it rose to 95°. It was then laid flat on short grass, when it fell to 60°; and on replacing it in its former position, in 4′ it again rose to 94°. On the 29th of the same month, at 10′ before 3 P. M., the same thermometer which had been exposed all day in a sheltered corner, rose to 150°. At the same instant another instrument, similarly prepared, and resting in contact with the herbage, indicated only 119°. Again, on the 29th, at 2 P. M., the first thermometer as before, was at 140°, and the second 110°. We have here a difference of 30°, arising solely from the manner in which the instruments were exposed. These trials are so few in number, that, were it not for the remarkable uniformity of the differences, it might appear unreasonable to apply them to any other observations. But it cannot be denied, that such a correction would at least render Captain Sabine's observations more closely comparable with those made at London. So far, then, the hypothesis appears to have advanced on trivial grounds.

In support of his idea, that the energy of the solar rays is diminished as we approach the Equator, the narrative of Humboldt is referred to. "I have often," says that illustrious traveller, "endeavoured to measure the power of the sun, by two thermometers of mercury perfectly equal, one of which remained exposed to the sun, while the other was placed in the shade. The difference arising from the absorption of the rays never exceeded 6°.6 Fahr." It is unnecessary to mention, that naked thermometers are quite unfit for such experiments. For of the rays which impinge upon a naked ball, all except those having a perpendicular incidence, will be reflected from the surface. Hence, the amount of heat developed must be extremely small; and during a breeze *, if the instrument be not screened from its effects, it is nearly neutralized.

We have made numerous observations with naked thermometers, but none of them deserve notice, except those made during the hot weather about the end of last July 1825. On the 27th, at 3 P. M., when the black thermometer was at 150°, a naked thermometer, exposed fully to the sun during a dead calm, rose to 99°; the temperature of the slates at this time was 117°, of the earth 103°. On the 28th, temperature of the air 62°, wind E., pleasant breeze.

At noon, black therm. 125°, naked 75°.
— 1 P. M.———— 139,———— 92.
— 2 P. M.———— 139,———— 90.
— 2 30′ ———— 135,———— 87.

On the 29th, temp. of the air 62°.5, wind as before. At 1 P. M., black thermometer 127°, naked 79°.

We covered the latter loosely with a piece of black cloth till it rose to 97°; on removing the cloth, it fell in 5′ to 83°. At 2 P. M., black thermometer 140°, naked 95°.

Temperature of the earth 101, of the air, three inches above the slates of a low out-house facing the south, and sheltered from the wind, 90°.

From these it appears, that, at a certain excess of temperature, the emission from the shaded part of the ball, owing to the high radiating power of the glass, more than counterbalances the calorific effects from the absorption. We see, therefore, that

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* These [illegible] not made at Cumana as Mr Daniell says, but on shipboard, dur [illegible] Teneriffe to Cumana: consequently they cannot be considered [illegible]

unless due allowance is made for the excess of temperature, the velocity of the wind, length of time of exposure, &c. we cannot get even an approximation to the true force of solar radiation. But in the hot countries, much higher temperatures have been obtained with naked thermometers, than any yet mentioned. In Caffraria, Mr Barrow saw an exposed thermometer mark 106°. The missionary Campbell, during his interesting journey in the winter time through the country of the Botchuanas, when the air at 8 A. M., saw the thermometer in the sun at noon rise to 84°. At Gondar in Abyssinia, Mr Bruce mentions, 113°; while at Benares, 110°, 113°, and 118° respectively, are recorded. There is another class of observations to which Mr Daniell has not alluded, though entitled to more confidence than those with naked thermometers. The force of the sun's rays may be conveniently ascertained, by taking the temperature of the surface of the earth, where it has been fully exposed to the effects of insolation. Observations of this kind are very interesting, but they are unfortunately few in number. We shall here cite some of those best authenticated.

In Sierra Leone, Dr Winterbottom saw a thermometer, placed on the ground, rise to 138°. Humboldt gives many instances of the temperatureof the earth being so high as 118°, 120°, and 129°; and at one time he found the temperature of a granitic sand, loose and coarse-grained, 140°.5; another, finer and more dense, 126°, the thermometer in the sun being at the same time 97°.16. "It is probable," he observes, "that the mean temperature of the dried mud, in which the alligators bury themselves during their state of periodical lethargy, is more than 104° Fahr."; that is to say, at least equal to the mean of maximum temperatures registered by Mr Daniell's black thermometer. Now, the mean of all his observations on solar radiation, including the summer of 1822, is only 79°.4. Fahr. We do not recollect any observations of this kind in our latitudes, except that mentioned above, where the temperature of the slates was 119°. Mr Coldstream informs us, that, on a very hot day in June last year, he found the temperature of the surface of an oil-painted garden seat, in a sheltered spot, with the sun beating upon it, to be 120°.

When we bear in mind the distinction made above, all the arguments which have been adduced for a remarkable intensity of solar radiation in high latitudes, will be found to amount to very little. There are no direct experiments made with proper instruments, but there is one fact which, it is confessed, may enter into close comparison with those made with the black-wooled thermometer. Captain Scoresby states, that, in the month of April, while, on one side of his ship, water was freezing rapidly; on the other side, which was exposed to the direct rays, the pitch about the bends of the vessel became fluid; while a thermometer placed on the black paint-work rose to 80°, or even 100° *. From this, however, must be deducted the influence of the light reflected from the surface of the snow and ice. We have no means of ascertaining how far these reflections did influence the observations; but it is well known, that, at the angle at which the rays impinged upon the snow at that time of the year, almost the whole of the incident light is reflected, without producing any elevation in the temperature of the snow and ice. In the month of April, in London, the maximum effect recorded is 110°, which is probably nearly double the actual amount here indicated. That distinguished traveller Sir Charles Giesecké made several trials with thermometers at Godhavn, in Lat. 69°. In calm and clear weather, the maximum he ever obtained was in

April,	61°	July,	89°.
May,	65	August,	89.3
June,	90.5	September,	63.5.

The last argument which has been brought forward, is derived from the experiments of Mr Knight, on the culture of the pine-apple. This able physiologist suggests, that the fruit will ripen better early in the spring than in the summer months. For, he says, this species of plant, though extremely patient of a high temperature, is not by any means so patient of the action of very continued bright light as many other plants, and much

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* Mr Daniell found, by experiment, the melting point of pitch to be about 120°; but we [illegible] Mr Scoresby's own account, as there are many compositions used in [illegible] all confounded under the general name of Pitch, such as boile [illegible] of oil and common pitch, pitch and ochre, &c., all differing [illegible] onsistence and fusibility.

less so than the fig or orange tree; possibly, having been formed by nature for intertropical climates, its powers of life may become fatigued and exhausted by the length of a bright English summer's day in a high temperature. When we reflect on the constitution of the natural climate of the pine-apple, we can easily understand the utility of this suggestion. For whatever be the intensity of the scorching rays to which the plant is exposed in its native country, the long sleep through a tropical night is sufficient to restore its energy. On the contrary, in the fruiting-house, in which the heat is as great as ever experienced in the Brazils, it is exposed to a blaze of light during a summer's day of seventeen hours; while, on the other hand, the short and imperfect refreshment which it can receive in a midsummer's night, is by no means sufficient to restore its active powers. It is certain, that, if more pains were used to equalize in this, as in other respects, the situation of the plants with that of their native soil, botanists would have it more frequently in their power to examine the fructification of many plants which, at present, shew no inclination to put forth their flowers.

Within the Tropics, the productions of the vegetable kingdom are never endangered, by any interruption in the regular alternation of atmospherical variations. The undeviating regularity in the succession of the agents which influence organized beings, induces extreme sensitiveness in plants to small changes in the condition of the circumambient medium. Hence, when the temperature of the air declines towards evening, the irritability of the plants is excited by the approach of cold; and, before the sun is set, flowers have closed their petals, and the delicate pinnated foliage has collapsed to present a further loss of heat by radiation. Even in the Temperate Zone, in those parts where continental climates prevail, or climates distinguished by a great difference between the summer's heat and the cold of winter, as in Russia, and in the central lands of Asia and America, plants are endowed with a similar constitution. Being subject during the winter to a degree of cold far below that at which their vital powers are suspended, they acquire a high organic susceptibility to the stimulus of light and heat, so that no sooner is the frost relaxed, than vegetation is renewed with a force and celerity unknown in this country. It is on this account that the mildness of our moist and changeable winters, proves so destructive to mountain plants, and not, as many have said, because the covering of snow under which they are buried in their native sites, protects them from excessive cold. We thus see why, in the Arctic Regions, when plants are awakened into life by the return of the sun, they resume their organic functions with such amazing energy, that they spring, flower, and ripen their seeds, in the short space of six weeks.

Our readers are now in possession of the leading facts, which are well authenticated, and it is for them to judge how far the first two questions have been satisfactorily answered. If we find Mr Daniell's to be without foundation, it is but fair to acknowledge, that the force of radiation from a vertical sun is not so excessive as might have been supposed. We are still unable to give any solution to the most important of the questions proposed, What is the maximum calorific impressions which plants are subjected to in any latitude? Nor have we ascertained the force of the sun in any place on the surface of the earth.

The experiments for this purpose are too delicate for ordinary hands, and, in our variable climate, more than one revolution of the season might take place, before an unexceptionable opportunity might occur. Agriculturists and florists are well aware of how much consequence the agency of direct light is in the flowering of the Cerealia, and the brilliancy of ornamental plants. The absence of this important agent, as Mr Daniell observes, can never be compensated for by any elevation of temperature under a clouded sky. It is also well known, that, in many years in which the harvests are nearly ruined, the average temperature does not fall below the ordinary mean of the year or of the season. It is therefore highly important, that journals should be kept, in order to ascertain the effects of this powerful element in different years. For this purpose, the best arrangement that can be adopted is that used by Mr Daniell, with the exception of giving the instrument a free exposure to the wind: For the object being merely to ascertain the total amount of radiant matter which plants have received during the day, the thermometer ought [illegible] as possible in the same condition with the foliage [illegible] of the plant.

The terrestrial radiation of caloric has been treated of at great length, and in a very interesting manner, in the latter part of the essay, and a comparison is also taken of the amount of nocturnal radiation in different latitudes. It appears that the nocturnal terrestrial radiation in the Torrid Zone falls short of what might have been expected; from which Mr Daniell concludes, that the same cause which obstructs the passage of radiant heat in the atmosphere from the sun, opposes also its transmission from the earth into space. While we assent to the unexpected nature of these results, we do not think it necessary to insinuate, with M. Gay Lussac, that they were obtained at times when the air was less clear or less calm than at London. There are two circumstances which ought to have been taken into account, and which are sufficient of themselves to explain the anomaly. First, the high temperature of the soil, which, in the Torrid Zone, frequently retains a heat several centigrade degrees above that of the air, even when the latter has reached its minimum. The second and most efficient principle is the law which has been established by Mr Anderson, That the minimum temperature of the night is regulated by the constituent temperature of the aqueous atmosphere. The enormous quantity of moisture in the atmosphere equally prevents the diminution of its temperature beyond a certain degree, and checks the cooling of the ground by evaporation.

ART. XII.—*Sketches of the Comparative Anatomy of the Organs of Hearing and Vision.* By THOMAS BUCHANAN, C. M., Author of the Illustrations of Acoustic Surgery, &c. &c. Communicated by the Author [*].

1.—*Ear of the Squalus.*

THE organ of hearing, in the shark tribe, varies considerably from that of the human subject.

We find neither ossicula auditûs, tympanum, eustachian tube, nor cochlea; but, as if to compensate for the want of parts so essentially necessary to the perfect ear, the semicircular canals are of almost incredible extent.

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[*] Read before the Wernerian Society, 19th Nov. 1825.

The cranium being composed of semi-transparent cartilaginous substance, of a beautiful cerulean hue, the whole of the organ can be distinctly seen, by merely removing the cuticle, and some of the surrounding parts.

When the cranium is divested of the common integuments, a considerable depression is seen in the coronal and posterior portion, which, in the living subject, is occupied by a kind of spongy elastic cellular membrane.

This depression is of a heart-like figure, the apex pointing forwards, with a roundish process in the posterior part, which causes a partial division of this portion of the cavity.

In the older fish, the intermediate space in the depression between the cuticle and the cranium, is chiefly filled with a transparent gelatinous substance, which can easily be seen and felt in the recent subject, by passing the finger backwards over the depression, when the posterior part will become tumid, like a bladder.

In the posterior, and rather inferior, and external parts of the depression, are four foramina, two of which are situated on each side of the posterior process.

The foramen next to the process, is large, and of a circular figure, and in the recent subject covered with a membrane, the plane of which forms an angle of nearly 45°, with a line through the center of the cranium, and about the same angle from the perpendicular. This membrane may, with propriety, be termed the *Membrana Labyrinthi.*

The spongy elastic membranous substance is attached to the external side of the membrane; but in the skate there is a considerable space between it and the substance.

This large foramen leads into the vestibule and posterior semicircular canal; and may be termed the *Foramen rotundum*, from its circular figure.

Close to, and outside of this foramen, is a very oblong aperture, through which the tube of the ear (in the young subject) enters t[illegible]ule, and which, from its extreme oblong figure, may be [illegible] *ramen oblongátum.*

In th[illegible] of the species *S. canus*, the meatus aulitorius externus is situated on the superior and posterior, or coronal, surface of the head, is small and much contracted, and seldom admits of any substance larger than a strong bristle.

The tube is formed of tough, elastic, membranous substance, and runs outwards or laterad, forwards or antinead, and downwards or basilad, a few lines more or less, according to the size of the animal, until it reaches within a short space of a membrane, stretched across the tube, where it enlarges to nearly twice the size of the other parts of the tube.

This membrane may with propriety be termed the ***Membrana Vestibuli.***

The tube then describes an angle by running downwards, backwards, and a little outwards, until it reaches the foramen oblongatum, to the edge of which it is attached in some subjects more closely than in others.

In young fish of the *S. canus*, the meatus and auditory tube are more easily found than in the adult, or in some of the other species. In old fish, the meatus is generally almost obliterated; the tube and membrana vestibuli are seen, but indurated and enlarged so as to be scarcely recognisable by the above description.

This alteration of the parts may perhaps be the consequence of disease, or the effect of old age. In the ear of the adult of the ***Balæna Mysticetus***, I have frequently found the stapes so firmly attached to the foramen ovale, that the union resembled ossification, and required considerable efforts to separate the bone from the foramen. The probable cause of this adhesion will be pointed out when describing the ear of that animal, so that one fact may, if possible, throw light on another.

The following are the dimensions of the parts in a preparation of the ***S.*** *canus*, three feet in length.

Distance between the orifices of the Meatus auditorius externus,			**2½ lines**
Diameter of the tube at the external orifice,		- -	**$\frac{1}{20}$ of a line.**
Do.	**do.**	**inside of the cuticle,** - -	**½ do.**
Do.	**do.**	**Membrana Vestibuli,** - -	**1 line.**
Length of the tube from the orifice to the Membrana Vestibuli,			**1½ do.**
From that membrane to the Vestibule,		- - -	**1½ do.**
Depression in the Cranium, in length about		- -	**6 do.**

In the species *S. borealis*, or Greenland shark, the magnitude of the semicircular canals is such as to surpass any idea which may be formed of the parts, from the dissection of the organ in the human subject. The superior size of the parts may be estimated from the dimensions of a cast of the left ear of that animal now before me, which I took a few hours after it was killed *.

The entrance of the meatus internus is about three lines in diameter, and situated in the inferior part of the organ.

It runs downwards, and a little outwards, about two lines and a half; then contracts suddenly, and runs horizontally outwards nearly a line, when it enters the vestibule, parallel with the floor of that cavity.

The vestibule is large, of an irregular triangular figure, the perpendicular of which may be said to present to the brain, while the base runs horizontally outwards and backwards.

The circumference of the greatest diameter of the vestibule is twenty-five lines, and the height of the cavity, from the highest to the lowest points, twenty-two lines.

On the inside of the superior part of the external angle of the vestibule is a longitudinal ridge, which, in the sketch of the cast, is seen as a depression; and on the floor, there is a correspondent ridge on the opposite side, that runs upwards on the parietes, next to the brain, until it arrives at the top of the cavity, where it unites and forms a septum, which separates the foramen oblongatum from the foramen rotundum.

The floor of the vestibule is more tough and hardened than any other part of the labyrinth, particularly that which is directly under the sabulous body, when it has a white, scaly, opaque appearance, approaching towards ossification.

The whole of the vestibule is lined with a reflection of the dura mater, which is closely attached to the parietes of that cavity, where it is considerably less dense than in the inside of the cranium, and still less in the cartilaginous, semicircular canals, where it is almost pellucid in the adult fish, and beautifully transparent in the young.

There are three semicircular canals, which arise from, and communicate with, the vestibule, similar to those of the human subject, and, from their relative situations to that cavity, may be terme [redacted] ior, Anterior, and External or horizontal.

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* and 2. where the parts are shewn the natural size.

The extremities of these canals, adjoining to the vestibule, are considerably enlarged, so as to have a similar appearance to that which in the human subject is termed the *Ampullæ;* whereas the proper ampullæ of these cartilaginous canals are situated at a considerable distance from the vestibule, and are of an oblong figure.

The Posterior canal is the largest, and measures *fifty-three lines*, or nearly five inches and a-half in length, and two lines by two and three quarters in diameter. It runs in a longitudinal direction from and to the vestibule, and, when viewed internally, has the appearance of a circle attached to the ampulla of the external canal.

The circumference of the ampulla is sixteen lines, and the average circumference of the other parts of the tube eight lines and three quarters.

The External canal is the next in size, and measures thirty-six lines in length and seven lines in circumference. The ampulla of this canal, at its greatest circumference, measures only ten lines and a-half.

The canal runs in a diagonal direction, the one extremity arising from the superior and posterior parts of the vestibule; and the other from a globular space communicating with the inferior and anterior parts of that cavity.

The Anterior canal, although the smallest of the three, is yet of considerable size, measuring thirty-one lines in length, and from five to seven lines in circumference.

These canals are partially flattened, so that if any of them were divided at a little distance from the ampullæ, the section would be of an oval figure, with the apex pointing inwards.

In a considerable number of preparations now before me is one of an elephant (the animal has apparently been of great age), where the caliber of the canals, one in particular, is extremely oblong.

When dissecting the organ of hearing in various animals, as well as in the human subject, I have found the caliber of the canals in children, young animals, and birds, to be circular, and in the adult ear, one or more of the semicircular canals slightly flattened; and this oblong form of the caliber increased considerably in aged persons, in whom it is seldom wanting. The apex in these cases invariably pointed towards the vestibule; indeed this position seems to be general, whenever the form of the caliber approaches towards an oval.

Seeing, then, that the caliber of these canals is uniformly circular in the young, of whatever class or species, and that, in the adult, one or more of the canals are generally more or less of an oblong figure, according to the age of the subject; and that this elongation of the parietes of the canal does not diminish the original diameter, and also that the caliber of the membranous semicircular canals in the old, as well as in the young, is circular, and always continues so when in a healthy state, notwithstanding any alteration which may take place in the form of the caliber of the osseous or cartilaginous canals; may it not be inferred from these circumstances, that the oblong figure of the caliber is caused by the vibratory action of the membranous tubes exciting absorption of part of the cartilaginous or osseous parietes of the canals in which they are inclosed, and that, by the continuance of this excitement, the oval form of the caliber is gradually increased?

This view of the subject will receive additional confirmation, if we take into account the myriads of vibrations which the membranous tubes perform in a few years. But how much the more immense must be the number performed by those of the aged, whether the person has frequented the busy haunts of the metropolis, or the more peaceful calm of rural solitude!

Such are the dimensions of the cast, taken with the greatest care, which will be sufficient to demonstrate the superior magnitude of the cavities to those of the human ear; and the following account of the contents of the vestibule and canals, the result of a series of dissections of the membranous parts and nerves in various species of the squalus, will, I hope, tend to place Comparative Anatomy, as regards these organs, in a more advantageous point of view, than usually esteemed by many of the profession *.

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* The Plate explanatory of the structure above described, will be given in the next numb[illegible]rnal.—Ed.

(To be continued.)

ART. XIII.—*On the Constancy of the Level of the Sea in general, and of the Baltic Sea in particular.*

ABOUT the middle of the last century, an animated controversy took place among the natural philosophers of the north of Europe, regarding the alleged gradual lowering of the level of the sea in general, and of the Baltic Sea in particular. Celsius was the first who introduced this idea to notice. He generalized it, by applying it to all the planets, and was supported by the authority of the celebrated Linnæus. It was soon perceived, however, that the point could never be settled by mere discussion, and that facts alone could lead to any certain result. Observation was therefore had recourse to; and thus, the dispute in question had at least one good effect, that of directing to the subject the attention of men of science, whose situation might enable them to mark the variations of level that take place along the coasts of the North Sea. The results of investigations, undertaken for this purpose, are now beginning to be collected.

In the course of 1820 and 1821, Mr Bruncrona, assisted by the officers of the pilotage establishment, and other qualified persons, undertook the examination of all the authentic measures that had been established upon the west coast of the Baltic during the last half century. The results of this examination are given in a short memoir inserted in the Swedish Transactions for 1823. The following Table indicates the degree to which the level of the sea has fallen during the last forty years, on the coast of Sweden, at various latitudes. It is proper to remark, that, in some of the places observed, the measures were much older, and in some others much more recent, than the period of forty years. In both these cases, the change of level that must have been effected during this period, has been estimated by calculating the mean annual depression furnished by the observations.

Latitude. East Coast.	Fall of Surface in 40 years, in feet.	Latitude. East Coast.	Fall of Surface in 40 years, in feet.	Latitude. East Coast.	Fall of Surface in 40 years, in feet.
63° 59′	1.50	59° 17′	2.17	56 10	0.00
— —	2.50	58 44	1.00	56 11	0.00
— —	0.50	58 42	1.08	55 53	0.00
61 43	2.50	58 45	1.17		
61 37	2.83	58 35	2.00	South-West Coast.	
61 32	2.50	58 28	0.07	55 23	0.00
61 45	2.50	58 11	0.83	55 22	0.00
60 11	2.33	58 8	1.00	57 21	0.00
59 46	0.17	57 50	1.00	57 53	1.00
59 46	2.00	56 41	0.41		

Of the facts collected in the course of this investigation, the following may be mentioned as tending to support the opinion of a fall of level.

1. It is generally believed among the pilots of the Baltic, that the sea has become shallower along the course which vessels ordinarily follow; but it is added, that this alteration is more sensible in the places where the tide collects sand, detached pebbles, and sea-weed, than in those where the bottom is composed of rocks. The same observation has been made in the neighbourhood of some large towns and fisheries; for example, a hydrographic chart, made in 1771, gives six fathoms for the mean depth of the sea opposite the harbour of Landskrona, whereas, in 1817, the sounding line scarcely gave five fathoms at the same point.

2. According to the oldest and most experienced pilots, the straits, which separate the numerous islets scattered along the coast of Sweden, from Haarparanda to the frontiers of Norway, received vessels that drew ten feet of water; now they are not practicable for boats that draw more than two or three feet.

3. The pilots further affirm, that, along the whole coast of Bothnia, the depth of the water undergoes a diminution, which becomes sensible every ten years, in certain places where the bottom is composed of rocks. Several other parts of the Baltic may be cited, in which a similar change has been remarked.

Mr C. P. Hallstrom, in an Appendix to Mr Brunerona's Memoir [illegible] he following Table of the diminution observed in the c [illegible] ters of the Gulf of Bothnia.

PLACES.	Mean marked in	Height of the water re-observed in	Fall beneath the original mark in feet.	Number of years.	Fall of the water in 100 years in feet.
aholem, parish of Lower Kalix,	1770	1750	2.05	50	4.10
		1775	2.49	75	4.32
or Rebben, parish of Pitea,	1751	1785	1.70	34	5.00
		1796	1.90	45	4.22
atan, parish of Bygdea,	1749	1785	2.70	36	4.72
		1795	2.50	46	5.43
		1819	2.60	70	3.47
	1774	1785	0.55	11	5.00
		1795	1.16	21	5.52
		1819	1.60	45	3.57
	1795	1819	0.65	24	2.71
Rönnskat, on the coast of Wasa,	1755	1797	1.70	42	4.05
		1821	2.87	65	4.35
Wargön, on the coast of Wasa,	1755	1785	1.45	30	4.83
		1797	1.69	42	4.02
		1821	2.87	65	4.35
Lögfrundet, near Sefle,	1731	1785	2.90	54	5.37
		1796	2.17	65	3.34
Ulfon, in Angermanland,	1795	1822	1.58	27	5.85

It is not demonstrated that the numbers of the last column represent exactly the lowering of the water in a century; for it has not yet been sufficiently determined if this lowering be uniform, or if it vary at different periods, and if it depend upon some local circumstance, upon the climate, or upon the state of the atmosphere. Nor is it properly established that this lowering, which becomes less perceptible from the north of the Baltic, until it disappears entirely at the southern extremity, follows precisely the same law of diminution as the latitude. It appears to be uniform in the whole extent of the Gulf of Bothnia, and it rises about four feet and a quarter in that region. At Calmar, (Lat. 57° 50′) it is only two feet, but it is not yet known whether it decreases in a regular manner between these two places.

Some authors consider the facts related by MM. Bruncrona and Hallstrom, as deciding the question in favour of those who believe in a lowering of the level of the Baltic. The editor of the Annalen der Physik *, goes farther, and seems to consider it as confirming the opinion of a general lowering of the level of the sea. In support of this opinion, he adduces the traditions and observations of the natives of Otaheite, and of the Moluccas and Sunda Islands, regarding the retreat of the sea in several parts of their coasts. We are disposed to stand neutral in this matter. The geographers * who have collected the greatest number of facts relating to the level of the inland seas, and of the ocean in its various regions, find nearly as many in favour of a rise as in favour of a fall of level. The very distribution of contrary indications leads them to believe in a partial displacement of the mass of waters from one region towards another, and even from the one side of an inland sea towards the opposite side; a displacement which might be owing to fugitive or more or less durable causes, such as a variation of temperature in the Polar Regions, the action of winds and of currents, modified by the greater or less quantity of water in the rivers that feed the different basins, upon the sides opposed to their direction.

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* 1824, St. 12. p. 443.

Are the facts contained in the memoir in question of a nature to overthrow this opinion? They do not appear so to us. The two series of observations which are adduced, only shew a fall upon the coasts of Sweden properly so called; that is to say, upon the west coast of the Baltic, and the east coast of the Cattegat. Two observations only have been made upon the coast of Finland, towards the extremity of the Gulf of Bothnia. These facts would perfectly accord with the opinion of those who think that the currents determined from the north to the south of the Baltic by the numerous streams which rush into it, push the waters toward the south shore, that of Pomerania, Mecklenbourg, and Holstein, and that the waters consequently gain upon the land on this coast, as numerous historical facts attest, while they retire along the northern shores,—those of the Gulf of Bothnia. Be this as it may, the question as to the constancy of the level of the sea cannot be considered as decided, until a long series of observations shall have been made upon authentic and perfectly fixed measures, erected upon all the shores of the different seas, and of the different regions of the ocean. Those which have been published in the Swedish Transactions, furnish

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* M [illegible] e la Geogr. Univers. tom. ii. p. 459. Catteau Calleville, Tabl. d [illegible] p. 158-188.

important documents for this purpose, and similar ones should be begun to be collected in other countries*.

ART. XIV.—*On certain Circumstances connected with the Condensation of Atmospheric Humidity on solid surfaces.* By HENRY HOME BLACKADDER, Esq., Surgeon. Communicated by the Author.

THE condensation of aqueous vapour from the atmosphere on the surfaces of solid bodies, is one of the most common and familiar of physical phenomena. Common and familiar as it is, however, there are circumstances connected with it, which render it not merely curious, but highly interesting, and which have attracted the attention of many celebrated natural philosophers. A number of facts have accordingly been well ascertained, and in explanation thereof, various theories have been proposed,—built, all of them, more or less ostensibly, on hypothetical bases. Electricity, radiation of heat, frigorific rays, and aërial pulsations, have each had their full share of attention. It is not the present object to attempt to decide on the individual merits of these theories, but rather to draw attention to some facts and circumstances which seem to merit farther consideration.

1. A number of experiments were made by Dr Wells, and more recently by others, on the condensation of aqueous vapour, by exposing pieces of gilt or silvered paper in the open air, after sunset. Now, though paper thinly coated with a metal, may be well fitted for experimenting on the spontaneous condensation of moisture, when the object is to ascertain the modifying effects of certain combinations, surely nothing could be less accurate than to reason upon such experiments, as if a thin plate of metal, and paper thinly coated with metal, were one and the same thing. Paper is one of the worst conductors of heat, and is, besides, highly susceptible of being influenced by atmospheric humidity. Hence, when placed in close contact with a sheet of

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* Bibliotheque Universelle, July 1825.

metal, not perfectly continuous, and much thinner than itself, it must greatly influence the latter, both in regard to the admission and discharge of heat, and that more especially in the circumstances necessarily connected with the experiments in question. Experiments made with gilt or silvered paper, therefore, cannot, with any pretensions to accuracy, be brought forward, as if they were equivalent to experiments made with thin sheets of polished metals,—and any reasoning that may have been built upon them under such an impression, must go for nothing. I may observe, that when paper, to which a thin layer of gold, silver, or other metal has been made to adhere, is thoroughly embued with varnish, the phenomena presented by the condensation of vapour are obviously modified; but still, as we had reason to anticipate, they are not the same as when a thin sheet of metal has alone been employed *.

2. For the purpose of ascertaining the degree of cold supposed to be produced by the radiation of heat, and, on other occasions, the amount of heat produced by direct solar radiation, it has most commonly been the practice to surround the bulb of

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* Hygroscopic substances of an animal or vegetable origin, cannot be entirely deprived of moisture, by a degree of heat short of that which is sufficient to produce a change in their chemical condition. When, therefore, a hollow ball of polished metal or of glass, containing a heated fluid, is observed to cool more quickly when covered with muslin, and suspended in the air, than when the balls have been left naked, is it sufficiently evident that vaporization has no influence in expediting the discharge of heat? When, again, a heated ball of metal is observed to part with its heat more quickly, when its surface has been covered with successive layers of gold-beater's leaf, than when only one layer has been applied, is it demonstrable that evaporation is in no degree operative? Perhaps it is not too much to take for granted, that no two hygroscopic substances absorb equal quantities of moisture in equal times; and, admitting this to be the case, we may conclude that they also part with moisture with different degrees of facility. May not the different degrees of velocity, therefore, with which heat is observed to escape from a polished metallic ball, according as its surface is covered with muslin-paper, gold-beater's leaf, glue, with or without pigment, &c., depend on some other circumstance than merely a difference in the mechanical form or structure of the surfaces? It is certain, that hygroscopic substances, when in that state commonly considered dry, are still far from being wholly deprived of moisture. If, when the atmosphere [illegible] a moderate degree of humidity, the temperature of a hygroscopic [illegible]ed considerably above that of the air, the substance will

a thermometer with wool, &c. or to ascertain the temperature of such substances, after they have been exposed, for a certain time, to the open sky, after sun-set, or to the direct influence of the sun's rays.

It must be admitted, however, that all such experiments are necessarily and in no small degree defective ; and whatever the results may have been, they can never prove that which they were intended, and have been supposed to establish. When it is received as a rule, that "no more causes are to be admitted than are sufficient to account for the phenomena," it must also be admitted, that, when two or more causes are immediately operative, the effect cannot be attributed to any one or more of these, to the exclusion of the rest. In the experiments referred to, all the substances made use of, such as wool, cotton, silk, lint, down, saw-dust, straw, &c. are not only bad conductors of heat, but of that description of substances which, according to circumstances, absorb, or give out moisture to the atmosphere,' with the greatest facility. Admitting, then, that, in certain circumstances, bodies, at the surface of the earth, did radiate their heat, so as to become colder than other bodies in contact with them, when experiments are brought forward to prove this effect of radiation, it is indispensably requisite to shew, either that evaporation was in no degree operative, or that its effects were in no degree proportionate to the observed decrement of heat. In this point of view, by far the greater number of Dr Wells's experiments seem altogether unsatisfactory, in as far as they were in-

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part with a portion of its moisture; but, sooner or later, a period arrives, when it ceases to become drier. If, at this period, however, we bring it into a body of air that is considerably drier, but of the same temperature, and still keep its own temperature equally above that of the air, we find that it gives out an additional quantity of moisture. If, lastly, we replace it, other circumstances being the same, in a body of damp air, we find that it regains a certain quantity of moisture. Is there not here a certain resemblance to what takes place when the temperature of a body is diminished by the process of evaporation? In the one case, there is a loss of heat until an equilibrium is established, that is, when as much heat is supplied by the air, as is carried off by the aqueous vapour. In the other case, there is a loss of moisture until an equilibrium is effected, that is, when as much moisture is absorbed as at the same instance escapes with the portion of air that is rarified?

tended to prove, that that loss of heat which frequently occurs in the evening, is the effect of *radiation*. He seems to have proceeded under the impression, that, in the circumstances of the case, evaporation could not have place; or, if occasionally it had, that its effects were but transitory, and of trifling import: and this appears the more remarkable, when we attend to some of his own experiments and observations.

Thus, on the evening of the 25th of August, he informs us, " 10 grains of wool, to which 3 grains of water had been added, having been laid on the raised board, near the thermometers; at the end of 45 minutes the parcel was found to have lost $2\frac{1}{2}$ grains of moisture, during the time that dry wool," that is, wool to which no water had been added *, " had become several degrees colder than the air." It is to be regretted, that, in recounting this experiment, more attention was not paid to minute detail, such as the temperature of the water made use of, the mode in which it was added to the wool; the temperature of the moistened as well as of the other parcels of wool, at the end of the 45 minutes, and their relative temperatures, at various intervals, during that period. For, without paying attention to every circumstance, even though apparently trifling, and without admitting every circumstance to have its due weight, it may truly be said of an experiment, that which has, with too much apparent justice, been said of a certain book, " Hic est in quo quærit sua dogmata quisque; atque in quo reperit dogmata quisque sua."

Again, " on the 7th of January," Dr Wells informs us, " 10 grains of wool were placed on a sheet of pasteboard, which lay on the snow. At the end of 35 minutes the wool was 5° colder than the air, without possessing any additional weight." But the evaporation of a very small quantity of moisture, from the surface of the wool, during the 35 minutes' exposure to the air, would be quite equal, in the given circumstances, to produce the observed decrement of heat. The object of this, and some other experiments, was to determine the occurrence of a considerable degree of cold, previous to any deposition of moisture, in the form of dew. When it was ascertained that the wool had not acquired any additional weight, no farther attention seems to have been paid to it; and as the experimenter had a previous conviction, that evaporation had no influence in producing the diminished temperature of the wool, the loss of weight must have been very apparent indeed, that would have arrested his attention. But it is well known, that the evaporation of a very minute quantity of moisture is sufficient greatly to reduce the temperature of the evaporating surface. And we may also remark, that the greatest degree of cold always takes place on those evenings when dew is latest in forming; that is, when the air is driest, and, consequently, when evaporation is necessarily most active. It must also be observed, that, in performing experiments with a nicely adjusted balance, even in a close room, accurate results are not to be obtained without considerable trouble. If, then, such an instrument be employed in the open air, on a damp evening, or in a cold benumbing state of the atmosphere, considerable inaccuracies must be almost inevitable.

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* Dr [redacted] here informs us, that the wool he made use of in his experiments [redacted] lerately fine, and already imbued with a little moisture;" and he a [redacted] uring his experiments, the wool might acquire some moisture [redacted] g it as a hygroscopic substance."

On another occasion, Dr Wells informs us, that, "on the 25th of January, the ground being covered with snow, during eight hours that I attended to my thermometers, the whole sky was constantly overcast with clouds. The atmosphere was, for the greater part of that time, very still; and a thermometer on the snow was generally about 2° lower than another in the air. That this was not owing to evaporation, was proved by the thermometer on the snow always rising, from a half to a whole degree, whenever the air was a little moved, and falling the same quantity as soon as a great stillness again took place." Far from proving, however, that the reduction of temperature was not the effect of evaporation, this observation will be found to furnish, if not a proof, at least a strong argument, in favour of that explanation. When the air was very still, that is, without a perceptible progressive or undulatory motion, the evaporation that was going forward at the surface of the snow carried off a greater quantity of heat than was communicated by the contiguous air. Hence the snow became colder than the air a short distance above it; an equilibrium being on this occasion established, when the temperature of the former was reduced about

2° below that of the latter. When, however, from some temporary cause, the surrounding atmosphere became agitated, that is, when a progressive, undulatory, or convolving motion had been communicated to it, the air contiguous to the snow was thereby either mixed with, or altogether displaced by, the adjacent air of a higher temperature. By this means the snow acquired an accession of heat, and the thermometer in contact with it indicated an increase of temperature. As long as the atmosphere continued agitated, fresh portions of air would every instant be brought into contact with the snow; and in this way supplies of heat would be furnished equal to that which was carried off from the snow by the evaporating process. When stillness again took place, though the air contiguous to the ground was not absolutely at rest, fresh parcels of the higher adjacent air were not now, as formerly, brought incessantly into contact with the snow; and hence the latter did not receive a quantity of heat equal to the whole amount of that carried off by the vapour, until its temperature was again reduced about 2° below that of the air, a few feet from the ground. I shall here merely introduce an experiment of Mr Howard. On a night, when the minimum temperature was 19°, that gentleman exposed 1000 grains of snow, on a dish 6 inches in diameter, and in the course of the night 60 grains were lost by evaporation. I have repeatedly made observations and experiments similar to those above adverted to, but it seems quite unnecessary on the present occasion to multiply examples. I shall therefore conclude this part of the subject with noticing an observation to be met with in the writings of a well known meteorologist. He informs us, that 'a ploughed field is more affected by the sun's rays than a plot of grass; because a loose spongy body, by exposing numerous surfaces, dissipates more quickly the heat communicated to it;' and, in confirmation and illustration of this opinion, he adds, that 'the inferiority of a grassy surface was not owing to the waste of heat by a more copious evaporation; for that, on spreading a layer of hay, or even wool, over a part of the naked soil, th[illegible]rature of it was in a few minutes reduced to the same [illegible] of the grass.'

Ha[illegible] as already been remarked, are hygroscopic bo[illegible]nductors of heat; and they are rarely met with in a state that can, with any attention to accuracy, be termed dry; more commonly they are in some degree damp. Besides, their temperature, when laid on the ploughed field, would, in all probability, be somewhat lower than that acquired by the surface of the bare earth, exposed to the direct influence of the sun's rays; and would, consequently, be lower than that of the aqueous vapour issuing from it. Independently, therefore, of their hygroscopic property, and of their mechanical operation, afterwards to be adverted to, they might thus acquire an accession to the moisture which they previously contained; and portions of this moisture being carried off by the contiguous air, their temperature would, in a few minutes, be reduced to that of an adjacent field of grass, and it might be in certain circumstances even lower.

It is generally admitted, that when the temperature of a body is considerably higher than that of the contiguous air, it will lose heat, both by conduction and radiation, or by some process equivalent to the latter. But as even a current of air cannot cool a perfectly dry body, below its own temperature, when any body is found to be colder than the air, the question to be determined is, Whether the loss of heat is to be attributed to evaporation, or to some process equivalent to that which has been termed *radiation?*

In the case referred to, the sameness of temperature in the grass, the hay, and the wool, is to be attributed to their being equally bad conductors of heat, and equally capacitated for supporting evaporation; and that not only from one exterior or upper surface, but from numerous interior surfaces, to which the air had access, and from which the sun's rays were more or less perfectly excluded. The naked soil, on the other hand, though comparatively a dense solid, and a good conductor of heat, has but one evaporating surface; while its dark or nearly black colour, enables it to absorb a greater proportion of the sun's rays, and convert them into heat of temperature, than bodies of a white, pale-yellow, or green colour. Hence the temperatures of the grass, hay, and wool, were somewhat less than that of the naked soil; relatively less heat being abstracted from the latter, by the process of evaporation, than it acquired through the influence of the sun's rays.

3. It has been well ascertained, that if, on a clear evening, for example, when bodies on the surface of the earth have become colder than the air, a cloud should pass over the zenith, the thermometer will indicate an increase of temperature, and, after the cloud has passed, it will again indicate a loss of heat. This increase of temperature has been accounted for in various ways. Some have supposed the heat to be evolved by the condensation of the aqueous vapour constituting the cloud; but it has not been satisfactorily explained how this heat is brought down to the earth, even admitting that such a quantity is evolved, as to render it appreciable beyond the immediate limits of the cloud, which, though it may be comparatively low, is still at a great distance from the earth.

This increase of temperature during the transit of a cloud, has been accounted for on the pulsatory hypothesis, by supposing, that "clouds, like water, absorb and extinguish all the hot and cold pulses received by them."

But were it even ascertained that "cold pulses," or "frigorific rays," were actually, "in some way or other, showered down from the upper regions of the atmosphere upon the earth," the phenomenon in question could not, it is presumed, be satisfactorily accounted for on that principle. For, a comparatively small cloud in the zenith, could not be supposed capable of neutralising the effect of the "cold pulses," showering in all directions from an extensive clear sky, by which the cloud is every where surrounded. We are informed, that the "cold pulsations" come with equal force from all quarters of the heavens, and at every angle 20° above the horizon.

Dr Wells was of opinion, that "no direct experiments can be made, to ascertain the manner in which clouds prevent, or occasion to be small, the appearance of a cold at night upon the earth;" but he concludes, "that they produce this effect, almost entirely, by radiating heat to the earth, in return for that which they intercept in its progress from the earth towards the heavens." This is the explanation originally given by M. Prevost of Geneva, and which is grounded on his hypothesis regarding radiation. "[illegible]louds," Dr Wells continues, "near the earth, must p[illegible] heat as the lower atmosphere, and will therefor[illegible]th as much, or nearly as much, heat as they receive from it by radiation. But similarly dense clouds, if very high, though they equally intercept the communication of the earth with the sky; yet being, from their elevated situation, colder than the earth, will radiate to it less heat than they receive from it, and may, consequently, admit of bodies on its surface becoming several degrees colder than the air." But dense clouds, though at times they may be *comparatively* near to the earth, never (excepting, perhaps, on some very extraordinary occasions) approach within a great distance of the low plains,—their elevation being commonly such, that, at that height, the temperature of the air must be very considerably below that of the lower atmosphere;—otherwise, the fact, now generally understood to be well ascertained, that the temperature of the air diminishes about 1° for every 300 feet of elevation, would be incorrect. Where the basis is so purely hypothetical, it is more surprising that the theory which is built upon it should be made to account, plausibly at least, for so much, than that it should seem to fail in some points, confessedly not free of obscurity.

When we observe a cloud passing at some considerable distance above the earth, and surrounded on all sides by transparent air, we are apt to imagine that the increase of moisture is confined to the space within the circumference of the cloud. This, however, is not necessarily the case. The body of air occupying the interval between the cloud and the surface of the earth, during the passage of the former, may be more humid than that body of air which preceded it, and than that which follows next in succession. And when we consider what the source is, from which the moisture of the atmosphere is originally derived, we can readily comprehend how this state of increased humidity may extend from the surface upwards. When the temperature of solid bodies at the surface of the earth increases during the passage of a cloud, the cold produced by evaporation is diminished, and this may proceed from the passage of a more humid body of air; the upper boundary of which is so moist, as necessarily to produce a cloud at that elevation. Again, as every portion of the stratum of air next the earth is not necessarily, and at all times, of equal temperature, and equally damp, especially when there is not a steady current of some force and duration, the increase of temperature indicated by thermometers suspended in the air, and lying on the grass, may proceed from the passage of a moister body of air, of a higher temperature, part of the aqueous vapour being condensed into a cloud at its upper boundary.

Fully to illustrate this view of the subject, it would be requisite to enter upon a wide field, still requiring cultivation. For there are abundant reasons for believing, that the formation of clouds is a subject still very imperfectly understood.

At present I shall only remark, 1*st*, That, on the occasions referred to, the cloud is always connected with the lower stratum of air, and the increase of temperature is always most apparent when the cloud is comparatively low and dense. 2*d*, That, when the cloud is high, and unconnected with the lower stratum of air, no change of temperature is observed to take place. 3*d*, That the change from a lower to a higher, and from that again to a lower, temperature, always infers a progressive motion of the air. The body of air over the place of observation is not stationary, its place being occupied by other bodies of air which pass in succession. 4*th*, That the increased temperature, if not influenced by the passage of a more heated body of air, never exceeds, and but seldom equals, that of the ground. 5*th*, That, during the increase in the temperature of the air, there is a decrease of the cold caused by evaporation ; and the change in the latter usually greatly exceeds that in the former. 6*th*, That, when the temperature of bodies at the surface of the earth has been observed to increase during the passage of a cloud, the moisture of the air has also been observed to increase, by means of a hygroscopic hygrometer *. From these and other considerations, it is inferred, that the increase of temperature on the occasions referred to, though usually attributed to the influence of the cloud, may have quite a different origin, and that the presence of a cloud may be merely a contingent circumstance, dependent on, and indicative of, a greater degree of moisture in that portion of air that is for the time incumbent over the place of observation.

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* An expansion hygrometer of extreme sensibility, may be constructed, by arranging a number of sentient slips in a form similar to the strings of a harp, but of equal lengths, and so connected, that the united expansions and contractions of the whole shall be pointed out by an index. One instrument of this kind, which I had constructed, and which was left with a friend on the Continent, possessed great sensibility ; its range comprehending only two ordinary hygrometric degrees, though its scale was several inches in length ; but by a simple contrivance, its index could be readily adjusted to any degree of a common hygrometer. Such an instrume[illegible]ly unfit for the more usual purposes of hygrometers, but it is admirabl[illegible]ng slight or transient changes in the state of atmospheric h[illegible]nent of this kind has been observed to continue for

(To be continued.)

ART. XV.—*Account of a Case of Poisoning, caused by the Honey of the Lecheguana Wasp.* By M. AUGUSTE DE ST HILAIRE *.

ARISTOTLE, Pliny, and Dioscorides, inform us, that, at a certain time of the year, the honey of the countries in the neighbourhood of Mount Caucasus, rendered those who had eaten of it insensible. Xenophon and Diodorus Siculus relate, that, at the siege of Trebisond, the soldiers of the army of the Ten Thousand ate of the honey which they found in the fields, and that afterwards they experienced a delirium of several days, some of them resembling drunken people, and others madmen, or persons in the agonies of death. Some modern writers have confirmed these statements, and have discovered that it is the flowers of *Azalea pontica*, and perhaps also those of *Rhododendrum ponticum* †, that communicate deleterious properties to the honey of Mengrelia. On the authority of the celebrated Tournefort, Lambert says, that the honey collected upon a certain tree of Colchia occasions vomitings. Tournefort himself ‡ asserts, that a constant tradition has established, among the inhabitants of the coasts of the Black Sea, a belief that the honey extracted by the bees from the flowers of Azalea pontica is dangerous. Lastly, a later traveller, Guldenstaedt, the companion

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a length of time alternately expanding and contracting, at short and irregular intervals, similar to what may have been observed when a manometer, having a great range, is fixed on the outside of a window.

* *Annales du Museum National.*

† M. Labillardiere supposes, that the cases of poisoning caused by the honey of Asia Minor, might be owing to *Menispermum Cocculus.*

‡ Voyages, ii. p. 228.

of Pallas, has himself seen the honey collected upon the *Azalea;* he found it of a dark-brown colour, and having a bitter taste; and in several places of his works, he says that this honey causes giddiness, and produces insensibility *.

Asia Minor is not the only country in which honey of a dangerous quality has been found. Roulox Barro, in his Voyage to Brazil, expresses himself on this subject as follows: "The most inebriated of the Tapuies searched for wild honey and fruits, of which they make a beverage, which is called *grappe*, and of which, whoever drank, immediately vomited." In the island of Maragnon, the bee *Mumbuca* sometimes, according to Piso †, rests upon the flower of the tree *Tapuraiba*, and then its honey, which is ordinarily delicious, becomes entirely bitter. Azzara is still more precise; for he expresses himself as follows in his Voyage to Paraguay: "The honey of a bee named *Cabatatu*, produces violent headach, and causes a degree of inebriation at least as great as that brought on by spirits. That of another species occasions convulsions, and the most violent pains, which terminate at the end of thirty hours, without producing any troublesome consequence. The country people are well acquainted with these two species, and abstain from their honey, although its taste is as good as that of the others, and its colour is the same."

The honey of Pennsylvania, of South Carolina, of Georgia, and of the two Floridas, when it has been gathered upon *Kalima angustifolia*, *latifolia*, and *hirsuta*, and upon *Andromeda Mariana*, often occasions, according to Smith Barton ‡, vertigoes, to which succeeds a delirium, varying in character according to the individuals. "The persons poisoned, adds the same author, "experience pain in the stomach, convulsions, vomitings, and sometimes these accidents are followed by death."

It is not alone in Asia and America that examples have occurred of poisoning, caused by certain sorts of honey. Seringe relates, that two Swiss herds who had eaten honey gathered from *Aconitum Napellus* and *lycoctonum*, experienced violent convulsions, and were seized with a horrible delirium; and that one of them, who was not able to vomit, died, emitting foam by the mouth, tinged with blood *.

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* Reis. i.

† Bras. 56.

‡ In Nich v. p. 159-165.

So many united authorities were not, doubtless, unknown to those who, even in our own times, have treated as fabulous the recitals of the historian of the Ten Thousand; but if these recitals needed a fresh confirmation, it would be found in the fact which I am about to relate, and which occurred to myself. To make myself better understood, I shall first give an idea of the places in which the event took place, from the fatal effects of which I narrowly escaped.

After having traversed the smiling plains of the Rio de la Plata, I had coasted the less inhabited banks of the Uruguay, and had come to the Camp of Belem, which occupied the site of the small town of the same name, destroyed by Artigas. Here I was informed that I should be obliged to cross a desert, where I should neither find inhabitants, nor traces of a path; but it was added, that, in case of need, I might have recourse to two detachments of Portuguese soldiers, posted upon the banks of the river; and I was willingly furnished with a guide to accompany me as far as the first post, placed toward the mouth of the Guaray. On the side of this river I exchanged my guide for another, who was to conduct me to the brook of St Anne, where I was told the second detachment was. When we arrived at this brook, we searched two days for the post of which we had been informed; but, finding that our efforts had proved unsuccessful, I sent back to the river of Guaray the guide who had conducted me to the brook of St Anne, and who had never been farther. I gave him one of the soldiers who had escorted me, to accompany him, and charged the soldier to bring me another guide. I remained waiting until they should arrive upon the banks of the brook, in a place which is now tenanted only by a multitude of Jaguars, and by immense herds of wild animals, deer and ostriches, opposite the right bank of the Uruguay, which was constantly traversed by bands of insurgent Spaniards at war with the Portuguese.

I had already been four days in this desert place, baffled by the rains which fell in torrents; discommoded by swarms of

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* Monograph upon the genus Aconitum, in the Mus. Helv. vol. i. p. 128.

troublesome insects, and having no other shelter than my cart, when at last the weather cleared up, so as to allow me to undertake a long botanical excursion. I took two of my people with me, and having armed ourselves so as to be able to keep off the Jaguars, should they attack us, we traversed the surrounding fields, and the banks of the Uruguay. At the end of some hours, hunger brought us back to the banks of the brook, and we assuaged it with our ordinary fare, manihot flour and cow's flesh, roasted and boiled.

During a short walk which we had made the day before, we had observed a wasp's nest suspended about a foot from the ground, from one of the branches of a small shrub. It was nearly oval, of the size of one's head, of a grey colour, and of a chartaceous substance, like those of our European wasps.

After dinner, the two men who had accompanied me upon my excursion, went to destroy the nest, and took away the honey. We all three tasted it. The person who ate most of it was myself, and the quantity which I took could not have exceeded two spoonfuls. I found it of an agreeable sweetness, and absolutely free of that pharmaceutic taste which the honey of our own bees so frequently has.

However, after eating it, I experienced a pain in the stomach, more disagreeable than acute. I lay down under my cart and slept. During my sleep, the objects dearest to me presented themselves to my imagination, and I awoke deeply penetrated with tender feelings. I rose up, but experienced such a degree of weakness as to be utterly unable to walk fifty paces. I therefore returned to my cart, and threw myself down upon the grass, when I immediately felt my face bathed in tears, which I attributed to a melancholy feeling produced by the dream which I had just had. Blushing at my weakness, I tried to laugh, but this laugh prolonged itself and became convulsive. However, I had still the power to issue some orders, and, in the mean time, my hunter arrived, being one of the Brazilians who had partaken with me of the honey, the baneful effects of which I now began to feel.

This m[illegible] the offspring of a Mulatto and an Indian woman, c[illegible] rare degree of intelligence, the most whimsical [illegible] all the levity which is peculiar to the

Mulatto. Frequently, after having experienced long accessions of the most lively and agreeable good humour, he was, without any apparent reason, seized with a gloomy melancholy, which lasted for some weeks, and, on such occasions, he found causes of irritation in the most innocent words, and even in the most delicate attentions. Jozè Mariano (for this was his name) came up to me, and told me, with an air of gaiety, and yet with somewhat of an odd expression, that half an hour ago he wandered in the country without knowing where he went. He sat down under the cart, and engaged me to take my place beside him. I had much difficulty in dragging myself so far, and, as I felt an excessive degree of weakness, I reclined my head upon his shoulder.

It was then that I began to experience the most cruel agonies. A thick cloud darkened my eyes, I distinguished nothing more than the figures of my companions, and the azure of the sky, traversed by some light vapours. I did not experience any great degree of pain, but I fell into the lowest state of debility. The concentrated vinegar which my people made me breathe, and with which they rubbed my face and temples, revived me with difficulty, and I experienced all the torments of death. However, I have perfectly preserved the recollection of all that I said and apprehended in these painful moments, and the recital which a young Frenchman, who then accompanied me, has since made to me, is in perfect accordance with my own recollections.—A violent combat took place in my mind, but it lasted only a few moments; I triumphed over my weakness, and became resigned to death. What affected me most, was the loss of my Botocudo Indian, whom I had taken from the woods, and who, I believed, would, after my death, be condemned to slavery. I conjured those who were about me to have pity upon his inexperience, and to inform my friends, when they should see them again, that my last prayers had been for this unfortunate young man. I felt an ardent desire to speak in my native language to the Frenchman, who lavished his cares upon me; but I found it impossible to recollect a single word that was not Portuguese, and I could not account for the shame and backwardness which caused this defect of memory in me.

When I began to fall into this singular state, I attempted to take water and vinegar; but having obtained no alleviation from it, I asked for tepid water. I perceived, that, as often as I swallowed it, the mist which covered my eyes was dissipated for a few moments; and I fell to drinking it at long draughts, and almost without interruption. I continually called for an emetic from my young Frenchman; but as he was confounded by all that was passing around him, he was utterly unable to find one. He was searching in the cart; I was sitting beneath, and consequently could not see him; however, it seemed to me as if he were under my eyes, and I reproaching him for his delay. This is the only error into which I fell, during the continuance of this cruel agony.

During these transactions the hunter rose up without my perceiving it; but presently my ears were struck with the frightful cries which he uttered. At this moment I found myself a little better; and none of the motions of this man escaped me. He tore his clothes with fury, threw them away from him, seized a gun, and fired it off. The gun was wrenched from his hands, and he then ran off into the country, calling the Virgin to his assistance, and crying out loudly, that all was on fire around him; that we were both abandoned, and that the trunks and cart would be suffered to be burnt. A Guarani workman, who formed part of my suit, having in vain attempted to keep him, was seized with terror, and took flight.

Until now I had not ceased to be carefully attended to by the soldier who partook, along with myself and the hunter, of the honey which had proved so baneful to us; but he had now begun to be very unwell himself. However, as he vomited very readily, and was of a robust habit of body, he very soon recruited his strength, which he did not, however, entirely recover. I have since found, that, while he was attending to me, he presented a frightful appearance, and was extremely pale. "I go," said he, all of a sudden, "to give notice of what is passing to the guard of Guaray." He mounted his horse, and galloped off into the country, but presently the young Frenchman saw him fall off; he got up again; galloped off a second time, fell again; and, some hours after, my people found him sound asleep in the place w[illegible]ad fallen.

I the[illegible], still almost in a dying state, left in company wi[illegible]ndian, who at best could render me no assistance, and by the young Frenchman, whom so many extraordinary events had, in a manner, deprived of reason. All the morning we had perceived insurgent Spaniards upon the opposite bank of the river; some of them even, who had crossed at a neighbouring ford, had shewn themselves, at a distance, upon the same side on which we were; and if they did not attack us, it was, without doubt, because they supposed us more numerous than we were. The dangers of my situation presented themselves in a lively manner to my imagination; and, weakened as I then was, I felt my malady still augmented.

I had calculated, that the soldier whom I had sent to Guaray would return this same day with the new guide. I flattered myself that I might receive some assistance from them; and my imagination divided itself entirely between the ardent desire of seeing them arrive, and the dread of the danger which I ran. I thought I perceived the dogs which accompanied my first guide; and the Frenchman assured me that I was not deceived. I thought they were returning with my soldier, and I felt myself reanimated with a glimmering of hope. But these animals quickly disappeared, and left me to all my uneasy feelings. They had been some of the brown dogs which wander in the deserts of the Uruguay; and having but little attachment to a master who fed them ill, they had without doubt been brought back by hunger to a place where they had been seen a few days before to worry a cow, of which we had given them a large portion.

During these occurrences, the hunter Jozè Mariano came and sat down beside me. He was calmer, and had passed a cloth about his waist; but he had not yet recovered the use of his reason. "My master," said he to me, "I have so long accompanied you; I was always a faithful servant; I am in the fire, do not refuse me a drop of water." Filled with terror and compassion, I took him by the hand, and, so far as my strength would permit, spoke some words of consolation and friendship to him.

However, the warm water, of which I had drunk a prodigious quantity, ended with producing the effect which I had hoped, and I vomited, along with a great deal of fluid, a part of the food and honey which I had taken in the morning. I then began to feel myself relieved. A rather painful numbness which I felt in my fingers, was of short duration. I distinguished my cart and the neighbouring pastures and trees: the mist, which had formerly concealed these objects from my eyes, only hid the upper part of them; and if it sometimes fell, it was only for a few moments. However this might be, the state of Jozè Mariano continued to give me great uneasiness; and I was equally tormented by the dread of never being able to recover the entire use of my strength and intellectual faculties. A renewal of the vomiting began to dissipate these fears, and procured me fresh relief. I had now still less difficulty in distinguishing the objects with which I was surrounded. I began to speak Portuguese and my mother tongue at pleasure; my ideas became more connected; and I clearly pointed out to the young Frenchman where he would find an emetic. When he had brought it to me, I divided it into three portions; and I vomited, along with torrents of water, the rest of the food which I had taken in the morning. Until the moment when I had discharged the last portion of the emetic, I had found a sort of pleasure in swallowing warm water at long draughts, but after this it began to produce a repugnance in me, and I ceased to drink it. The mist entirely disappeared; I drank some cups of tea, took a short walk, and soon found myself in my usual state.

Nearly at the same moment his reason suddenly returned to Jozè Mariano, without his having experienced any vomiting. He now assumed new habits, mounted on horseback, and rode off to look for the soldier, whom he presently brought back.

It might be ten in the morning when we all three tasted the honey which had proved so injurious to us, and the sun was setting before we found ourselves perfectly recovered. The momentary absence of the Frenchman and Botocudo Indian had preserved them from eating of the honey along with us. The soldier had presented it to the Guarani workman; but the latter knowing its deleterious quality had refused to take it. The Brazilian laughed at his fear, and did not even think that they should offer me part of it.

Ne[illegible] as still somewhat weak. The soldier complained of bei[illegible] ar. Jozè Mariano asserted, that he had not ye[illegible] trength, and that his whole body seemed to him as if covered with a glutinous matter. However, as our new guide had arrived the evening before, we set off betimes, in order to get away from a place which we could no longer look upon but with a kind of horror. Through the whole day, I found it impossible to think of any thing else than the events of the preceding day; and when we halted, I noted them down such as I have related them above.

I had said to one of my soldiers, that I should like to possess some wasps of the species which produced the honey, whose troublesome effects we had experienced. A little before arriving at the place where we put up the day after the accident had befallen us, I was called by the soldier, who shewed me a wasp's nest similar to that of the day before. It had the same form, the same dimensions, the same consistence; it was equally suspended from one of the lower branches of a small shrub; and my Guarani labourer, as well as the new guide, another labourer, and several Indians whom the guide had brought with him, recognised this wasp as belonging, like that of the preceding day, to the species known in the country by the name of *Lecheguana*. My soldier took possession of the nest, and brought me some of the flies, as well as fragments of their abode. The combs which I have sent, along with the wasp, to the Royal Cabinet, were similar to those which I had in my hands the day before; and the honey which they contained was of the reddish colour of that of the common bee, and was, like it, very fluid.

One may easily imagine the astonishment and chagrin which I experienced, when the soldier told me, that my Botocudo Indian, who had been a witness of the manner in which we had been affected, and the labourer of the guide, had eaten of the honey, and that their example had influenced my Guarani labourer. I could not help loading these men with all the marks of my indignation and disdain. " This honey will do me no harm, replied the Botocudo coolly to me, it is so sweet !"—words perfectly characteristic of the Indian, who is always full of the present, and never looks to the future.

Expecting a recurrence of the scenes of yesterday, I prepared emetics. I sent my people to lie down, and went to work in my cart. In a minute, all was in the most profound quiet around me. I awakened the Botocudo; he assured me he was exceedingly well, and the night passed without any accident.

As soon as I had got out of the deserts, in which I then was, and entered the province of the Missions, I asked a great many people about the honey of the *Lecheguanas*. All, whether Portuguese, Guaranis, or Spaniards, agreed in saying, that two species of Lecheguana were distinguished in the country; the one which affords a white honey (*Lecheguano de mel branco)*, and the other which produces a reddish honey, (*Lecheguana de mel vermelho)*. They added, that the honey of the former species never did harm; that that of the other, the only kind which I have seen, did not always do harm, but that when it did prove troublesome, it occasioned a sort of drunkenness or delirium, which were removed only by vomiting, and which sometimes went so far as to occasion death.

I was informed that the plant was perfectly well known from which the *Lecheguana* wasp frequently extracts a poisonous honey; but it was not shewn to me, and I was unfortunately left to form conjectures regarding it *.

ART. XVI. *Sketches of the extent of our information respecting Rail-roads.* By the Rev. JAMES ADAMSON.

WE must look upon the employment of iron surfaces upon roads, as only the natural consequence of the continual attempts to improve them, and as a thing likely to have been often talked of, and predicted, long before the advancement of art permitted the adoption of a material so expensive. To derive the greatest benefit from the methods of conveyance in use at present, would require the presence, on our roads, of two kinds of surface, of which neither can be found in perfection in any intermixture of the two; yet, in the formation of a public road, there is an attempt to combine the two incompatible qualities of presenting a hard and smooth surface to the wheels, and a soft and rough one to the feet of the horse. It is an obvious improvement to allot separate spaces to the differing surfaces. The ployment of hard-stone tracks, alternating with spaces cover- by a softer material, appears to have been an early step to- ards this separation; but the most advantageous form of it is ound in the modern iron rail-roads. It is generally considered, that the day's work of a horse on a rail-road, will be about 7½ times that of the same animal on a turnpike road; but I do not know that it has been accurately ascertained, what may be the proportional intensity of the resistance on the turnpike, in its best condition, or that we have at all the means of judging of the effect of substituting hard-stone tracks under the wheels. I should fear, that, though they may at first afford a tolerably appropriate surface, on which the resistance may be very much inferior to that presented by a turnpike-road, their good condition could not endure long. Every one must have observed the rounded form assumed by the upper surfaces of the square blocks with which streets are paved, and that the abrasion of the angles leaves ultimately a very irregular surface. The interstices will be found deeper in the direction across which the wheels generally move; since, from the elasticity of the paving material, and the ground which supports it, the angles of the stones are peculiarly exposed to the action of the wheels. We may expect that the same effect will be produced on stone-tracks; and that they will, to a certain degree, present an irregular knobbed or undulating surface, incomparably less advantageous for traction, than that of the more perfect material of the iron railways.

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* In t [illegible] f this Journal, we shall give M. St Hilaire's account of the variou [illegible] plants, which grow in the southern parts of Brazil.—ED.

The advantage possessed by stone tracks in admitting the employment upon them of wheels of the ordinary construction, is shared by the plate or tram rail-road; and this renders that form superior to others, for many purposes. These tram-roads seem to be almost universally in use in the mineral districts of Wales. This preference is approved of, but without assigning any adequate reason for it, by Mr Overton, an engineer of that country *. The only shadow of an advantage claimed for the system is, that it presents a greater resistance on descents where retardation is required. But this excess of retardation is continual; and it is certainly preferable to get rid of it, and to produce,

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* Account of the Mineral Basins, &c. of South Wales.

from other causes, the required increase of resistance, only in those situations where it is necessary. Mr Wood [*] determines, from experiment, the relative resistance on the plate-rail and the edge-rail to be as 73 : 63; and if, as is probable, the rails in those experiments were swept clean, this proportion must be more in favour of the plate-rail than that likely to be afforded by the average performance upon them; for the greatest disadvantage of the plate-rail is, that it is so much more apt to retain upon it those substances which increase the resistance. The suggestion of Mr Tredgold [†], that the angle formed by the plate and its ledge should be rounded off, will, I have no doubt, be found advantageous in practice, as it must tend to prevent the rubbing of the wheels upon the ledge.

The conclusion seems well established, that the edge-rail affords the most advantageous result, from the power employed upon it; but we still want, to a certain degree, the means of deciding on the comparative merits of the substances of which it is formed. I do not know that experiments on a great scale have as yet been made on any rail-roads, except those of cast-iron; so that the effect of diminishing the number of joinings, by using the longer bars of the malleable rails, is not exactly ascertained. But no one who has been dragged over both of them, or has inspected them together, can fail to give the malleable rails a decided preference. Of their comparative durability we must speak with more diffidence, until the facts be ascertained by experience; but I do not imagine that there will be found ultimately much difference in this respect. I had an opportunity of handling part of a bar, referred to in a discussion on this subject in the Newcastle Courant about a year ago. It had been in use as part of a rail-road about sixteen years, and except that the edges of the upper surface were considerably rounded off by the action of the wheels, it exhibited wonderfully slight appearances of decay. The Bedlington patent rails are merely a copy in malleable iron, as close-

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[*] A practical Treatise on Rail-roads, &c. by Nicholas Wood.

[†] A pr [illegible] Rail-roads and Carriages, by Thomas Tredgold.

When t [illegible] entlemen are quoted, the above are the works referred to.

ly as the manufacturing machinery will allow it, of that form which had been found most advantageous for cast-iron rails. There are two distinct parts of that form, for which it will be useful to have distinguishing names. The upper flat part, along which the wheel rolls, we may, from its analogy to the old wooden rails, call the *tram* of the rail; to the part projecting downwards from this, we may apply the appropriate designation of the *keel* of the rail. The keel is deepest in the middle between the two points, upon which the rail is supported. The vertical longitudinal section of the rail is therefore somewhat similar to the segment of a curve cut off by a chord. Now, as in the malleable rails, many such lengths are formed in one piece, the lower part has an undulating appearance; and the production of this irregularity in depth, is one of the most ingenious parts of the beautiful process of their manufacture. As it is done by an excentric groove in one roller revolving opposite to a concentric one in another, it is evident that this part of the process cannot be repeated; for the second attempt might merely shift the undulations to other parts of the bar. Besides the undulations thus produced in the lower part, there are slighter corresponding ones produced in an opposite direction, in the upper part of the rail. To straighten this upper surface, the rail is put several times through grooves in the rollers, which compress that part in all directions, but exert merely a lateral pressure upon the undulated under part. Thus, if there be any difference of texture in the different portions of the rail, the upper part will be more dense, and the under part will approach nearer to the condition of wire-drawn iron; and each will be of the nature best suited to resist the different kinds of action to which they are exposed. But as the whole process takes place on a short mass of iron, which is gradually rolled out to about six times its original length, and as the operation is finished before the metal has lost its red heat, it is not likely that there will be any perceptible difference of texture, or that, in uniformity or toughness, the rail will be in any way inferior to other malleable iron. There is thus little probability of the occurrence of that exfoliation which it is imagined will take place upon them by the effect of great pressure. Not the least appearance of it is to be seen on rails of this sort, which have had engines of about

7 tons in weight, in constant employment upon them for above two years.

The duration of the rail ought to be determined by the period during which the upper part retains sufficient thickness to support the pressure of the wheel, without being broken or folded down; and if it be found, that, in the malleable rails, the under part decays too rapidly, then, as much iron must be added, beyond what is necessary for the due strength of the rail at first, as will enable the keel to retain its requisite strength and stiffness, until the upper part be worn away. Though somewhat of the strength and stiffness be lost, in a form of uniform depth, compared with that which is deepest in the middle between the points of support, when the quantity of iron is the same, it may perhaps be found advisable to relinquish the vertical undulations in the keel; in order that less surface and fewer angles may be exposed to the influence of moisture. This would be most advantageously effected, by having a keel of uniform depth, expanded into a cylindrical form at its under surface. We should also, then, have a neat and convenient method of attaching the rails to the blocks on which they rest: for a cast-iron chair or pedestal, formed so as to embrace this cylindrical part, might be slid on at the end of the rail, and pushed along to its proper place, where it would keep hold of the rail without pins or wedges. This sort of chair could be so formed, as to obviate, to a great degree, the consequences of the partial displacement of the blocks. The rail would have also the power of expanding longitudinally, without producing any derangement, and thus, on straight lines, very considerable lengths of the rail might be welded together, without inconvenience.

The breadth of the tram of the edge-rail is never, as far as I have seen, above 2¼ inches; and no such rule is observed, as that which Mr Tredgold mentions, viz. "That the breadth in inches should be twice the weight upon one wheel in tons." The rule is founded on the circumstance, that the loaded coal-waggons, in the neighbourhood of Newcastle-upon-Tyne, generally weigh four tons, and the rails are almost always about 2 inches b [illegible] along the same rails roll the engines also, carrying [illegible] e weight of a waggon. In fact, the rails have be [illegible] eased to their present breadth, with the view of preventing them from cutting grooves in the wheels, and that breadth appears to answer well for the heaviest weights likely to be permitted on rail-roads.

The strange form of a rail-road, consisting of a single line of rails, supported on upright pillars, proposed by Mr Palmer, and recommended by Mr Tredgold, will, I suspect, be found applicable to few situations. It differs from others, in requiring not only room in breadth and height, but also a clear space of some feet below the rail. Thus, it cannot approach the surface of the ground without having a trench cut to receive it; and to secure a level line, must require either very high pillars in the valleys, or deep excavations through the hills. If it be not destitute of curves, the motion on it must be slow and regular, else the tangential force of the load will derange the structure; and if the pillars be inclined towards the centre of curvature, to counteract the effect of one velocity, that inclination will suit no other. If a continuous chain were attached to it, as Mr Tredgold proposes, it would meet with a very serious obstacle on all roads crossing it; for, except they should go over it by a bridge, it must proceed at a very expensive height above them, as it would then be impossible for a carriage to go through it.

Mr Wood has made the nearest approach to the complete elucidation of the data necessary to determine the resistance upon rail-roads, and the power requisite to overcome it. The experiments made by Mr Tredgold for this purpose, do not appear to be of much value, and in his book they are narrated too vaguely, to lead the reader to any decisive conclusion. In those which he has described, for the purpose of shewing the proportional resistances with different loads, and different wheels, the weights which produced the motion seem to have been omitted in determining the mass moved; and the real resistance, independent of the accelerative force of the moving weights, is not calculated at all. In the two experiments, from which he deduces the real amount of the friction at the axle of the carriage, the proportion of the gravitating force, employed in accelerating the revolution of the wheels, was probably of sufficient amount to require alteration. Mr Tredgold remarks, that "He avoided the smoothness and accuracy of workmanship," in preparing his model, "which could not be adhered to in machines in use." Now, this forms the strongest objection to any reliance on his experiments; for no model can have that relative smoothness, and accuracy of workmanship, which is found in larger machines. In constructing coal-waggons, which carry about three tons, the minutest attention is generally employed to secure accuracy of form, and smoothness of surface, in the moving parts. I had an opportunity of observing the importance of attending to those circumstances, while assisting Mr Wood to make some experiments at Killingworth, in August last. In applying some new bearings to new axles, though both had been turned and polished in lathes with the utmost care, yet the friction at the axle did not become constant, and reach its minimum, until the carriages had been dragged about heavily loaded, during a whole day. It requires attention to those circumstances, to account for the difference in the ratios of the friction to the weight, as determined by Mr Tredgold and Mr Wood, the one estimating the friction at double the amount which the other assigns to it.

In Mr Wood's table, representing the relative and actual resistances in different experiments, there is somewhat of embarrassing obscurity, arising principally from his not having narrated before, the whole circumstances of one of the series of experiments contained in it *. It is satisfactory to find, that the result of his two totally dissimilar methods of experimenting agree so closely. His deduction from them may apparently be depended upon in practice: That with wheels, of which the ratio of the diameter, to that of the axle, is 12 : 1, the total resistance will be $\frac{1}{200}$ part of the weight of the whole carriage and load. I have had an opportunity of witnessing experiments, in which, by taking every precaution to obviate the causes of retardation, it was reduced very considerably beneath the lowest amount in Mr Wood's table. We do not yet know exactly what proportion the resistance arising from the contact of the wheel with the rail bears to the whole; yet, as it is evidently very small, and probably diminishes as the diameter of the wheel increases, we may decide, that the fraction $\frac{1}{16}$ of the weight expresses with sufficient ac [illegible] sistance at the axle, and that this quantity,

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[illegible] say on Rail-roads, p. 195.

divided by the number of times the diameter of the wheel contains that of the axle, will express the whole resistance, when the machinery is in tolerably good order. This, when the ordinary wheels, three feet in diameter, are used, amounts to about 11½ lb. per ton; so that, if the constant progressive effort of any power be known, we can readily tell with how much it ought to be loaded on a rail-road.

There is a very great variety of opinion and statement with respect to the power of horses at different velocities. The experiments do not yet seem to be sufficient in number, and sufficiently varied, to afford unquestionable conclusions. The formula $(12 - v)^2$ seems to give the velocity corresponding to the maximum effect, a higher value than experience warrants. The Tables given by Mr Wood, of the performance of horses on the colliery railways, represent the effect of the horse as so very irregular, that they lead to no very satisfactory conclusions; because we do not know what effect such irregularities may have, in influencing the amount of the work done. His statement, that the power of a horse travelling 20 miles per day, at the rate of 2 miles per hour, may be represented by 112 lb. is more probably under the truth than above it. Mr Tredgold has pointed out, that the general formula ought to include in it as an element, the length of time during which the labour is continued; and that, corresponding to each assumed duration, there is a velocity which produces a maximum of effect, and that this velocity must have a certain relation to that rate of motion which a horse can sustain unloaded, during the number of hours assumed for the duration of his labour. But the assumptions from which are deduced the numerical values of those velocities, are either unintelligible to me, or are totally inadmissible. There results from them the strange position, that the muscular force which can be continued for a day, has to the weight of the animal the ratio of 3.37 : 1, though in fact the true ratio is more nearly the inverse of this. If, according to Mr Tredgold's estimate, a horse could exert a constant force of 125 lb. during 6 hours, at the rate of 3 miles per hour, his day's work on a rail-road would, at the rate of 11½ lb. per ton, be 10.8 tons conveyed 18 miles: according to Mr Wood's estimate, it would be 6.6 tons conveyed 20 miles. In these estitimates, and in those of Mr Wood's table, p. 239. the weight of the carriage is considered as part of the load, and this in general is rather more than one-fourth of the whole weight.

These theorems express only the relation of the effort to the effect, on a dead level. On an ascent, not only must the resistance be increased, but wherever the moving power resides in a moving body, the effect of its effort must be diminished. Thus, a horse weighing 10 cwt. walking unloaded up an ascent of 1 foot in 33, would exert an effort nearly equal to that of dragging 1 ton on a level rail-road. The weight of the moving body is peculiarly worthy of attention, when locomotive engines are employed. In the theorems on this subject, as they are stated by Mr Tredgold, the weight of the engine is not admitted as part of the load; but it bears too great a proportion to the whole load, to be safely neglected, and the introduction of it will be found to modify very greatly the practical conclusions to be drawn from the formulæ.

Let E represent the weight of the engine, and a be that fractional part of its weight representing the available friction which produces the progressive motion of the engine-wheels upon the rails; then E a will represent the engine's force of traction upon a level.

Let i be the angle of inclination;

W the weight of the waggons and load;

f the friction at the axle of the waggons when the pressure is 1;

n the diameter of the wheel when that of the axle is 1;

then we have the general equation to express the relations of those quantities,

$$E\,(a \mp \sin i) = W\left(\frac{f}{n} \pm \sin i\right).$$

The upper signs give the equation for ascending slopes, and the lower that required for descending slopes. W may be expressed by a multiple of E, and in that case we shall be able to find the inclination at which any required proportion of the work done [illegible] y be performed. If $\sin i = o$; then

$$E\,a = W\,\frac{f}{n};$$

and as, by Mr Wood's conclusions,

$$a = \frac{1}{25}, \text{ and } \frac{f}{n} = \frac{1}{200}, \text{ then } W = 8\,E,$$

or the proper load for an engine on a level is *eight* times the weight of the engine.

If we wish to know at what inclination the engine would retain only half its power, we may make $W = 4E$; then $\sin i = \frac{1}{250}$, or the ascent will be 1 foot in 250, or about 21 feet per mile. In this case, two engines would perform the work of one on a level. The use of two engines on such slopes, one acting in front of a train of waggons, and the other behind them, has been proposed by Mr Stephenson of Newcastle-upon-Tyne; and where the inclinations are of considerable length, would form a most convenient method of surmounting them. If, in the general formula, we make $\sin i = \frac{1}{250}$, and use the lower signs, we will find that, at that inclination, one engine will travel down with *forty-four* times its weight, or eleven times the load which it could drag up the ascent. By the same formula, if the effort of a horse, at any velocity, be represented by $\frac{1}{10}$ of his weight, he will, on a level, drag twenty times his weight; and the inclination at which his load, with the same velocity, ought to be one-half, or only ten times his weight, is $\frac{1}{206}$. The effort of a horse in carrying a load, is assumed to have, to his power of traction, the ratio of $3:1$; or $\frac{\sin i}{3}$ is substituted for $\sin i$ in the first member in the equation. This is on the supposition, that the friction of the carriages is as small as that which is created on rail-roads. If the friction on a common road amount to five times that on a rail-road, the load, in the same circumstances, will, on a level, be four times the weight of the horse; and the inclination, diminishing the load to one-half, will be $\frac{1}{46.6}$, or 1 foot in 47 nearly. Hence we see the necessity of diminishing the rate of ascent on public roads more than is generally done, as well as improving the surface.

The same formula will afford us the means of discovering what ought to be the inclination of a rail-road, when the traffic in one direction bears a known proportion to that returning in the opposite one. If we make the ratio of these loads, expressed as multiples of the weight of the engine, to be $q:1$; then, taking the values of $\sin i$ from the equations, with the upper and lower signs separately, we have the resulting equation,

$$\sin i = \tfrac{1}{2} \times \frac{q+1}{q-1} \times \left(a + \frac{f}{n}\right) \pm \sqrt{\tfrac{1}{4} \times \left(\frac{q+1}{q-1} \times a + \frac{f}{n}\right)^2 - \frac{af}{n}}$$

If $q = 2$, and the other symbols express the same quantity, as before,

$$\text{Sin } i = \frac{1}{666} \text{ nearly;}$$

in this case $W = 6$, and $qW = 12$; or an engine which, on a level rail-road, drags eight times its own weight of loaded carriages, will, on an inclination of 1 foot in 666, drag up six times its own weight, and will drag down twelve times its own weight. If $q = 4$, which is nearly the proportion when loaded carriages descend and empty ones alone return, the inclination required is about $\frac{1}{361}$; in this case the weight dragged up ought to be nearly 4.8 times the weight of the engine, and that taken down the inclination ought to be rather more than nineteen times the weight of the engine. If $E = 7$ tons, the weight of the empty carriages will be about 33½ tons, and the weight of the goods conveyed on them will be about 100 tons.

From the great effect which the weight of the engine and load, independent of their friction, has in diminishing the progressive effect on inclinations so small, we may perceive how little can be gained by enabling the engine to ascend greater inclinations; since we must make a great disproportion between either the loads, on a level and on an inclination, or their velocities.

(To be continued.)

ART. XVII.—*Table of Magnetic Variations.*

THE following Table, which contains a pretty complete enumeration of the late observations on the Variation of the Magnetic Needle, will be useful to those who may not have opportunity or leisure to consult the different works through which they are dispersed.

No. of Obs.	Longitude, W.	Latitude, N.	Magnetic Variation.	Year.	Observer.
			West.		
1	80° 51′	63° 27′	37° 30′	1824	Lyon.
2	48 9	59 49	48 38	1819	Parry.
3	61 59	63 44	60 20	...	...
4	62 8	63 29	60 56	...	...
5	61 50	63 58	61 11	...	...
6	62 8	63 26	61 23	...	...
7	62 9	63 29	61 50	...	...
8	59 12	70 29	74 39	...	...
9	57 36	73 23	80 1	1818	Ross.
10	57 56	74 1	80 30	...	...
11	59 56	72 0	80 55	1819	Parry.
12	68 37	70 22	80 59	1820	...
13	60 9	73 0	81 34	1819	...
14	60 11	73 5	82 3	...	...
15	60 12	73 3	82 37	...	...
16	63 0	75 51	87 50	1818	Ross.
17	61 5	75 32	88 13	...	...
18	64 45	75 50	90 18	...	...
19	64 41	75 59	91 17	1818	Ross.
20	71 18	71 16	91 29	1820	Parry.
21	64 43	75 50	91 33	1818	Ross.
22	65 40	75 55	92 44	...	...
23	72 54	76 30	103 41	...	...
24	80 8	74 25	106 58	1819	Parry.
25	77 1	76 33	107 56	1818	Ross.
26	77 22	73 31	108 47	1819	Parry.
27	78 48	76 8	109 1	1818	Ross.
28	89 22	73 11	114 17	1819	Parry.
29	88 18	73 33	115 37	...	...
30	89 41	72 45	118 16	...	...
31	91 47	74 40	128 58	...	...
			East.		
32	103 44	75 9	165 50	1819	Parry.
33	105 54	75 3	158 4	...	...
34	107 3	74 58	151 30	...	...
35	110 36	75 35	135 4	1820	...
36	110 27	75 7	128 30	...	...
37	110 49	74 47	127 48	...	...
38	110 34	74 47	126 17	1819	...
39	111 37	75 3	126 2	1820	...
40	111 55	75 13	125 15	...	...
41	111 57	75 5	123 48	...	...
42	111 12	74 9	123 6	...	...
43	111 42	74 28	117 52	1819	...
44	112 11	74 27	114 35	1820	...

No. of Obs.	Longitude, W.	Latitude, N.	Magnetic Variation.	Year.	Observer.
45	112° 41′	74° 25′	111° 19′E	1820	Parry.
46	112 53	74 24	110 54	...	...
47	113 48	74 26	106 7	...	...
48	115 50	67 43	50 20	1821	Franklin.
49	116 7	67 23	49 46	...	...
50	115 26	66 40	48 1	...	...
51	112 30	67 42	47 38	...	...
52	115 42	66 45	47 8	...	...
53	115 37	67 48	46 26	...	...
54	110 5	68 19	44 16	...	...
55	116 27	67 1	44 12	...	...
56	114 25	66 5	43 29	...	...
57	113 8	65 13	43 4	...	...
58	114 25	66 5	42 59	...	...
59	114 27	65 43	42 17	...	...
60	109 44	67 19	41 43	...	...
61	108 40	67 40	41 10	...	...
62	110 41	67 54	40 49	...	...
63	109 48	67 7	40 38	...	...
64	113 34	64 2	37 19	1820	...
65	113 3	64 15	36 51	...	...
66	113 47	63 47	36 45	1821	...
67	113 6	64 28	36 27	1820–1	...
68	114 9	62 17	33 36	1820	...
69	114 13	62 26	33 8	...	...
70	114 27	63 14	33 4	...	...
71	114 2	63 34	32 31	...	...
72	113 22	60 50	31 2	...	...
73	113 26	60 46	27 25	...	...
74	111 11	57 48	27 20	...	...
75	113 30	60 55	26 45	...	...
76	113 52	61 11	25 41	...	...
77	110 49	56 40	25 40	...	...
78	109 52	56 43	25 2	...	...
79	111 9	56 40	24 18	...	...
80	109 23	56 24	22 50	...	...
81	111 18	58 43	22 50	...	...
82	108 51	55 55	22 33	...	...
83	107 53	55 27	22 16	...	...
84	107 30	54 16	22 7	...	...
85	106 13	52 51	20 45	...	...
86	107 17	53 28	20 21	...	...
87	102 17	53 57	17 17	1819	...
88	100 44	53 27	15 20	...	...
89	97 41	54 12	15 0	...	...
90	98 1	53 42	14 26	...	...
91	96 17	54 29	13 20	...	...
92	96 16	54 31	12 47	...	...
93	96 1	54 39	12 45	...	...
94	95 22	54 51	12 40	...	...
95	95 0	54 59	11 50	...	...
96	94 21	55 14	11 10	...	...
97	94 26	55 12	10 28	...	...
98	93 30	56 4	9 28	...	...
99	93 2	56 22	9 5	...	...
100	94 21	55 14	8 40	...	...
101	93 57	55 17	8 30	...	...
102	93 52	55 29	7 48	...	...
103	92 26	57 0	6 0	...	...

Art. XVIII.—*Observations and Experiments on the Structure and Functions of the Sponge.* By Robert Edmond Grant, M. D., F. R. S. E., F. L. S., M. W. S., &c. Continued from Vol. XIII. p. 346.

Sponges grow so abundantly on our rocky coasts, and attain so considerable a size, that few animals can be said to present equal facilities of observing their natural habits, or of discovering their properties by experiment. Montagu, many years ago, described thirty-nine species inhabiting the British shores; and nearly half that number occur in the Frith of Forth, which will be mentioned individually in describing the skeleton of this animal, and the characteristic differences in the forms and arrangement of the spicula in different species. Almost every rock along our coast, placed near low-water-mark, supports some species of sponge; and, as far as my experience goes, the same is the case with every other shore visited by the waves of the ocean. I have found them growing alike on the sheltered transition boulders of the western shores of Italy, as on those which break the force of the tempests in the Bay of Biscay; and I have found the most delicate of our British species, spreading alike on the stupendous primitive cliffs of the Western Islands, exposed to the rapid currents and constant swell of the ocean extending to the American shores, as on the secondary rocks in the more sheltered bays of our eastern coast. The *Spongia papillaris* and *S. urens*, line the sea-worn cavities and fissures of quartz, gneiss, and granite rocks, on the western promontories of the island of Islay; and the same species spread over the sheltered hollows of the decayed greenstone columns on the coast of Dunbar. In the deeper parts of the Frith of Forth, the sponge so much abounds, as to encumber the dredges employed by our fishermen in collecting oysters, mussels and clams; and when thus torn from its native seat, by the long continued and daily operation of some hundred dredges, it is washed ashore alive on different parts of the coast in such quantities, that it is collected with other zoophytes and fuci, to manure the adjacent lands. So numerous are these animals in more southern latitudes, that the collecting of them for the demands of commerce, forms a lucrative profession, with many on the coasts of Calabria, Sicily, and the Grecian islands; and in a single voyage to the South Seas, nearly 100 distinct species were collected, which are preserved in the museums of Paris.

If the irritability of the living sponge, therefore, has hitherto escaped observation, that cannot be attributed to the rarity of the animal, although this circumstance has been complained of by Mr Ellis and some other naturalists; nor can it be ascribed to the smallness of the object, or the difficulty of examining it in its native retreats; for several species of sponge attain, even in our cold latitudes, a magnitude of some feet; and many of them grow so near the shore, that they are left exposed for two or three hours during the recess of every ordinary tide. At Leith and Prestonpans Bay, the *Spongia urens* or *tomentosa* is observed, in places accessible at low-water, spreading over the sides of rocks, and insinuating itself into all the interstices of the roots of fuci, without venturing to climb up along their stems; but, in deep and tranquil situations, where the fuci are less agitated by the waves, and attain a greater size, the *tomentosa* attaches itself directly to the stems of the large Fucus palmatus, along which it mounts to an extent of more than three feet, with a thickness often exceeding an inch from the surface of the plant to the external surface of the animal. It thus chokes up the superficial pores of these sturdy plants,—diminishes their supply of nourishment,—adds to their weight,—causes them to present a greater surface to the motions of the sea, and, in this exhausted condition, the plants are less able to retain their attachment to the rocks, or combat with the storms, which generally wash them ashore in great quantity, loaded with these large sponges in the highest degree of vitality. Sometimes the fishermen's dredges are brought up from deep water nearly half filled with bushes of the *Spongia coalita;* and specimens of the *Spongia dichotoma* are sometimes dredged near Inchkeith, measuring a foot in length, and nearly as much in breadth.

When two sponges of the same species come into contact with each other in the progress of their growth, they unite so completely, as not only to obliterate the line of junction, but even communicate freely by their internal canals. Thus I have seen a fecal orifice formed exactly at the place of the junction between two branches of the *Spongia oculata*, and communicating with both, although these branches were hanging by different stems and separate roots, from the roof of a small cave. When branches of the *Spongia xerampelina* or *ventilabrum*, or of the *Spongia prolifera*, are kept in contact with each other, by the washing up of stones against them by the tide, or by tying them together, they anastomose in the freest manner, and produce combinations of form, which render the distinction of branched species by that character extremely perplexing. This power of uniting is much more strikingly exemplified in the sessile species; for we frequently observe the side of a rock studded over with separate young *Spongiæ papillares*, not larger than a pea, which, in the course of a few weeks, unite into a continuous surface of sponge of more than a foot square; and it is amusing to observe the spreading and uniting of the young *Spongiæ parasiticæ*, on the back and legs of the living Cancer araneus, Pent., where they frequently collect to the number of forty or fifty, interrupt the joints of this lazy crab, spread like a mantle over its back, and, from want of space to creep upon, rise in fantastic ornaments upon its head, which the crab is unable to remove, from the small extent of motion admitted of by its hinge-like joints.

Different species of sponge do not unite together when they come into contact; they form a slight adhesion, but the line of separation is easily traced, and they can be disunited without laceration. When the *Spongia tomentosa* meets the *Spongia papillaris*, the margins of both adhere together, rise a little from the rock, and proceed directly outward, as if endeavouring to surmount each other, till their contest is arrested by the action of the waves, which would soon tear off the unsupported margins, if they proceeded outward to any considerable extent. This power of uniting, possessed by the individuals of the same species, is common to the sponge with plants; and, as is proved by uterine monsters, with all the higher orders of animals, and is the reason why we frequently find even the small sponges of the Frith of Forth covering a continuous surface of several square feet. As I have found sponges of such magnitude on various parts of our coast, accessible at ordinary tides, and very numerous during the recess of stream-tides, I am forced to differ from Mr Ellis, who states, in apology for his deficient account of this animal, that it is very rare to find sponges on our coasts, which have not been long removed from the places where they grew, and whose structure has not been very much injured. In so far as size, and number of individuals, and variety of species, are necessary to an inquiry into the structure and functions of this animal, the anatomist, who confines his investigations merely to the species of the Frith of Forth, will find no reason to envy the opportunities of Marsigli, Donati, and Olivi, who examined them on the shores of the Adriatic; of Jussieu, who examined them with the microscope on the coast of Normandy; Spallanzani, on the shores of the two Sicilies; Cavolini in the Bay of Naples; Lamouroux on the coast of Spain; Schweigger in the Gulf of Genoa; Bosc, Peron, and Lesueur, who examined them in Equatorial seas; nor of Peyssonel, who investigated the nature of the sponge on the shores of Europe, Africa and America.

Most sponges, like Thalassiophytes and the lower classes of marine animals, suffer, without inconvenience, the occasional privation of their natural element; and the species seem to possess this accommodating power in different degrees. The *Spongia dichotoma* inhabits very deep water near Inchkeith, and I have never seen it deserted by the tide; the *Spongia coalita* covers our oyster beds under twenty or thirty feet of water, and portions of it growing in their natural situation, are seldom deserted by the lowest tides; the *Spongia panicea*, and the *Spongia seriata*, (a species which, I believe, undescribed, and which I have so named, from the regular close ranges of fecal orifices which traverse its flat, smooth surface, and which are never raised to the extremities of projecting ridges, as in the *S. cristata*, but lie on a level with the general surface of the animal, as in the *S. panicea*, along with which it is found on the under surface of rocks; see collection in *College Museum*, Spongia seriata, *Gr.*), are found abundantly on rocks which are only left uncovered during the ebb of stream-tides, and are not accessible at ordinary tides; the same is the case with the *S. oculata*, *palmata*, *prolifera*, *xerampelina*, and *cristata*; the *S. urens* and *S. papillaris* on Leith rocks, remain for more than three hours uncovered during moderate tides; the *Spongia compressa*, which at Leith is comparatively rare, and remains only about an hour exposed to the atmosphere, is, at Prestonpans Bay, one of the most abundant and hardy species, hanging in thousands from the roofs of the most exposed caves, and remaining uncovered for three hours, during moderate tides. Although most of the species are thus periodically exposed to the air, this is no way necessary to their existence; for the same species which grow nearest to the shore, are likewise inhabitants of the deepest water, as is seen by their being cast ashore attached to stones, shells and fuci, after storms, and by their frequent appearance in dredges employed in deep-water; and we frequently find specimens of the *Spongia papillaris*, and *Spongia tomentosa* or *urens*, lining the sides of limpid pools during the retreat of the tide, where one-half of the animal above the surface of the water is subjected to long and regular visitations of the atmosphere, while the other half beneath the surface of the pool is never exposed to its influence from birth till death.

In all these sponges, we can not only perceive distinctly the currents rushing from the fecal orifices; but, with a little attention, we can likewise perceive, with the naked eye, the pores on their surface, by which the water enters into the internal canals; and, in some of the species where the pores are large, we can see, without the assistance of a glass, particles of matter drawn into the pores. I have not met with any kind of living sponge, in which the pores and fecal orifices were not visible, although one might be led to suppose, from the statements of Schweigger, and other naturalists, that some species of this animal want these openings, and are entirely covered with a gelatinous crust, through which water soaks by a kind of infiltration. When we place a branch of the *Spongia coalita*, in a watch-glass, with sea-water under the microscope, and look attentively along the side of the branch, at a distance from any fecal orifice, we see the small particles suspended in the water, beneath the surface, rush with an increasing velocity towards every part of the smooth surface of the branch; the smaller particles pass in and disappear, while the larger are arrested, and cling to the side of the sponge, where, in the natural abode of the animal, they would remain, till washed away by the ceaseless motions of the sea. A thin portion cut from the surface of the *coalita*, and viewed through the reflecting microscope, is seen to be every where pierced with polygonal pores, whose parietes are formed by fasciculi of straight, cylindrical, pointed spicula of considerable strength; and we can perceive particles of matter driven with some force through the pores of this detached portion of sponge. Were the branch of the *coalita* not completely under the surface of the water, the rush of particles to its porous surface might be mistaken for the result of cohesive attraction, as we see particles floating on the surface of water rush towards any dry, solid body in their immediate vicinity. If we raise the extremity of the branch of the *coalita* above the water of the watch-glass, and allow the fecal orifice to continue its current under the surface, we see that all the particles of dust that light on the surface of the water are quickly conveyed along to the exposed part of the branch, where they are either arrested from their size, or are seen to rush into the pores lying on a level with the surface of the water. From the pores, they pass down through the internal canals to the fecal orifice, which propels them to the surface of the water, to recommence their mysterious circulation. The pores of the living *Spongia coalita* are not very obvious to the naked eye; but are seen large and distinct over the whole surface of bleached portions found on the shore.

The pores of the living *Spongia panicea* are quite visible, without the assistance of a glass; and the canals and fecal orifices of this animal are uncommonly wide. In a portion of this sponge, placed perpendicularly in a glass of clear sea-water, I could perceive, through the sides of the vessel, with the assistance of a single lens, particles of matter distinctly drawn into the pores. On rubbing some powdered chalk lightly on the surface of the sponge, and replacing it in the water, I could see, with the naked eye, at the distance of six inches, some particles of the chalk, which still clogged the margins of the pores, successively driven into the interior, and disappear. One of the ova of this animal, swimming about by its own spontaneous motions, like the ova of several other zoophytes observed by Ellis and Cavolini, happening to come very near the surface of the sponge, I observed suddenly drawn towards the opening of a pore; and, from being too large to pass in, it was held in that situation for a time, by the entering current, till it disengaged itself, by accelerating the motions of the ciliæ which cover its surface: for the ova of this animal contain many distinctly formed spicula, and are not capable of moving by changing the shape of their bodies, as the ova of the madrepore, gorgonia, and sertularia are said to do, but are seen to swim about, by the rapid vibration of the ciliæ on their surface, while the shape of their body remains perfectly unchanged. The pores of the *Spongia panicea* are frequently obliterated in dried specimens, by the hardening of the gelatinous matter into an opaque membrane over the surface; and I have frequently produced the same appearance in other flat species, by drying them before their gelatinous matter had been sufficiently extracted by boiling water. This artificial covering resembles the gelatinous mantle of medusæ drying and hardening in the summer's sun, which we have seen strewed over many of the shores of Europe, and is probably the same with the compact crust spoken of by many naturalists as covering different species of sponge.

In most sponges, the currents through the pores, canals, and fecal orifices intermit, as we have seen above, without inconvenience, during every recess of the tide; for no fecal orifice that is above water is ever observed to pour forth a stream, even though the rest of the animal be entirely under the surface; and it is curious to observe, on the sides of pools, one-half of the animal under the surface, carrying on a circulation of water constant through life, while the other half above the water of the pool is subject to frequent and long intermissions.

A fecal orifice, raised only half above the surface of the water, produces a current which has a powerful effect on the particles floating near it. When a portion of sponge is confined in the same basin of water for about two days, the currents appear to have entirely ceased; but, on plunging it again into water newly taken from the sea, they are renewed in about two minutes, and continue nearly with their original force; but I have seldom kept sponges alive, in their adult state, for more than a week. I have frequently caused the ova to fix themselves on watch-glasses, and have reared them for a month. As far as I have been able to observe, the animal never intermits spontaneously the currents, and renews them again in the same water. In ceasing, they are observed to die away gradually; and no burning, or tearing of any part of the animal, causes them to intermit, though it hastens the period of their total cessation. A thermometer placed in the water, and another plunged to an equal depth in the substance of the animal, when the currents are in full activity, indicate no difference of temperature.

Having observed that the structure and disposition of the pores, canals, and fecal orifices of the sponge had an obvious relation to this circulation of water through its body, I could no longer doubt that the currents formed one of the living functions of this animal; and, as the existence of this living function was instantly ascertained, by placing the sponge in sea-water, and was so conspicuous as to be visible at the distance of ten feet from the animal, I employed it in all my succeeding experiments, whenever it was of the slightest importance to ascertain that my specimens were still alive. As I had already satisfied myself that the fecal orifices had no concern with the production of the currents, by observing that they continued the same, when all the papillæ were cut off, and finding it impossible to determine, from the discordant statements of naturalists, how far this function might depend on the contractile power of the animal, I performed several experiments to ascertain the extent of this power, in order to compare it with the force of the currents, and to observe how far the properties ascribed by the ancients to the sponges of the Mediterranean, agreed with those of the species now inhabiting the Frith of Forth.

I first selected a young branch of the *Spongia coalita*, which I judged, from the velocity of its currents, to be in perfect health; and, in order to observe it minutely, and at the same time to preserve it, as nearly as possible, in its natural state, I placed it in a shallow vessel, with some clear sea-water, in the light of the sun. On touching its body smartly with the finger, and observing it for five minutes afterwards, I could not perceive any trembling motion of the animal, or any gradual contraction of its body; it did not bend itself to either side, nor could I perceive any hollow formed at the place touched. When the surface of the *Lobularia digitata* is touched with the finger, there follows not only a retraction of the polypi, but, the zoophyte continuing to contract its fleshy axis, there is a slight hollow at length for [redacted] the surface, at the place where the finger touched. a needle through the body of the animal; and g it, I could not detect, with the assistance of a languid motion of the part, or of the branch, although the currents continued unaltered. On pouring off the water, I let fall a drop of nitric acid on the middle of this single branch; the corrosive poison sunk like water into the body of the animal, and, though again watched for five minutes, there was no perceptible shaking, or bending, or shortening of the sponge; nor could I observe any shrinking or depression on the place where the acid fell; that place of the branch quickly assumed a milky-white colour, while the rest retained its natural bright straw-yellow colour. When the *coalita* is young, its branches are long and slender; they shoot in all directions to seek for points of attachment, and adhere to, or envelope, every thing they meet with, living or dead, animal, vegetable, or mineral; wherever the branches cross or touch each other they form a perfect union; sometimes the animal spreads as a layer over an oyster-shell, or covers a rock like a convoluted bush, or like the root of a fucus, or forms a cement, connecting into a mass all manner of shells, stones, or broken glass; sometimes it forms an irregular mass, with a perfectly smooth surface, without any point of attachment, rolling to and fro, at the mercy of the waves. As it advances in life, its colour assumes a darker shade, with a tinge of brown; it becomes less smooth on the surface; loses its translucency; and its fibrous part predominates, as the hard parts of other animals predominate progressively from birth to decay. After storms, or during the dredging season, irregular branched masses of it are left at low water, along with the spatangus, and many other interesting animals, on the extensive sands of Musselburgh. I have frequently repeated the above experiments on the coarse, rough branches of the adult *coalita*, but with the same result; the acid seems partially to dissolve the part, and renders it at length more transparent.

I next took a portion of the *Spongia urens*, which formed a covering of nearly an inch and a half in thickness around a large stem of the *Fucus palmatus* or *digitatus*. It had been torn from the rocks in deep water, and was left on the sands by the retiring tide. Being perfectly entire, and uninjured, and some feet in length, I plunged its thickest extremity into a basin of water, to observe its currents, and touched the immersed surface with the finger, but no kind of contraction or trembling motion were perceptible; no dimple formed at the part touched. Having raised the immersed part a little from the water, after two pins had been thrust into its surface, parallel and near each other, I struck, with a red hot wire, the exposed part of the surface between the pins; but, what I little expected at that period of my inquiry, the parallelism of the pins was not disturbed, nor did they seem to approach each other in the slightest degree. Lest the pins might have approached each other, in a small degree, without disturbing their parallelism, I placed them on a part of the animal newly raised from the water, and measured their distances with a pair of compasses; but, after receiving some smart strokes with a red hot iron, on the surface, between the pins, half an inch distant, the points of the compasses still coincided with the heads of the pins; there seemed to be no more effect produced on the living animal than would be produced on a piece of common moistened sponge. The surface of the *urens*, when young, is somewhat transparent, and of a yellowish-grey colour; but, as it advances, it acquires a brighter yellow colour, and more opacity; when looked closely into, it appears covered with a net-work of the finest gauze, the pores being visible to the naked eye. I had hopes of inducing motion in these pores; but, on observing them through a glass, while I irritated them with a needle, I could perceive no change in their dimensions. When this sponge spreads on the sides of rocks, its fecal orifices are observed to be more raised from the surface than in portions of it surrounding fuci, corallines, or other moveable bodies; and they are likewise more thin and transparent on their margins; so that, when the *urens*, taken from such situations, is kept for a time out of water, the first parts which begin to collapse or contract, by drying, are generally these transparent lips of the orifices. This takes place equally in dead and living specimens, and might be mistaken for an effect of irritability; it is the only kind of motion I have ever been able to produce in these parts.

In Prestonpans Bay, the tide has excavated, in many places, the beds of soft slate-clay from beneath the outgoings of the sandstone strata, and has thus formed innumerable small caves which are sheltered from the direct force of the waves, by lofty ridges of trap-rocks extending to a great distance from the shore. In these sheltered recesses, far from the main current of the Frith, numerous species of *Alcyonium*, *Lobulariæ*, *Sertulariæ*, *Corallines*, *Tubulariæ*, *Flustræ*, branched sponges, and other zoophytes, dispute with different species of the *Doris, Ascidiæ, Actiniæ*, and myriads of the testaceous mollusca, the possession of a calm and secure retreat. At low water I have often punctured and irritated the ends of the branches of the *oculata, xerampelina, prolifera*, and *palmata*, while hanging, uninjured, from the roofs of these caves; but have never observed the slightest reaction or shrinking of any kind produced by the animal. I have tried the same on many sessile species covering the rocks, and with as little success. I have plunged portions of the branched and sessile sponges alive into acids, alcohol, and ammonia, in order to excite their bodies to some kind of visible contractile motions, but have not produced, by these powerful agents, any more effect upon the living specimens, than upon those which had been long dead.

Strange as these results may at first appear, in an animal of such magnitude and softness, I am happy to find that they perfectly agree with those obtained by the most eminent observers on the sponges of warmer latitudes. Bosc and Peron could not observe the slightest motion in any of the numerous species which they collected in their voyages. Spallanzani and Olivi, by puncturing and tearing the living animal, could not produce the smallest contraction. Cavolini could not produce the slightest shrinking of the animals, by piercing and handling many of them adhering to the rocks, under water, in the Bay of Naples, during a perfectly calm sea. Schweigger performed many experiments to discover the contractile power of the living sponge, but could not produce the slightest motion in those inhabiting the shores of the Mediterranean, although he was misled by Marsigli and Ellis to believe that the animal had the power of sucking in and squirting out water by the fecal orifices. I cannot therefore help thinking, that the naturalists of Torona, more than twenty centuries ago, and Aristotle, who seems to agree with them, came nearer to the truth in denying that the living sponge contracts itself, when touched, than Cuvier and Lamarck, who maintain at present a contrary opinion. In opposition to the observations of so many naturalists, Lamarck appeals to the testimony of the Greeks, in proof of a contractile power existing in the mass of the sponge. The testimony alluded to, is contained in the passage of Aristotle, inserted near the beginning of this memoir, where the contractions of the living sponge are mentioned by that author, merely as a matter of vulgar report (ὥς φασι), without adducing the authority of any observer; whereas he expressly states, that those who inhabited Torona did not believe in the existence of any such property in this animal. The naturalists to whom Aristotle refers, and with whose evidence he appears satisfied, from not mentioning those who were of an opposite opinion, had certainly the best means among the Greeks of observing the phenomena of this animal, in its living state, from their southern exposure and warm latitude, on the shores of Macedonia, only 17° from the Torrid Zone, and from their sheltered situation at the head of the present Bay of Cassandria, where the delicate zoophytes, which covered their rocky coasts, were protected from the tempests of the Ægean Sea, by the long and mountainous promontories of Pallene, Sithonia, and Athos.

From this extraordinary inertness of the sponge, under every circumstance, to the strongest artificial excitement; and from the circumstance shewn above, of its not contracting its body spontaneously, during the flow of the currents, we feel compelled to ascribe that function, for which the whole body of the animal seems so admirably constructed, to some powers which are incessantly in action, while the general mass of the zoophyte is at rest. We shall now try, if possible, to discover those moving powers which seem to contain the secret of this mysterious being; but, before entering on this new kind of investigation, it is necessary to give an outline of the internal structure of the animal, that we may enter, with more minuteness of detail and precision of language, into what relates to the functions of its individual parts.

(To be continued.)

ART. XIX.—*On the Detection of Boracic Acid in Minerals by the Blowpipe*[*]. By EDWARD TURNER, M. D. F. R. S. E. Lecturer on Chemistry, and Fellow of the Royal College of Physicians, Edinburgh.

IN the paper which I had the honour of reading before the Society at its last meeting, on the detection of lithia in minerals,

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[*] Read before the Royal Society of Edinburgh on the 19th December 1825.

I described three different fluxes, by means of which the presence of that alkali might be readily detected in spodumene and petalite. I had at first supposed, that any substance which enabled those minerals to fuse readily before the blowpipe would answer the same purpose; and though this notion proved to be erroneous, it was not altogether without its use. For among other re-agents that had been employed without success, I had used solid boracic acid, and mixtures of boracic acid with fluate of lime, and I observed that they uniformly tinged the point of the blowpipe-flame of a pure green colour, similar to what is seen during the combustion of alcohol in which that acid is dissolved. Hence arose the question, whether the same colour might not be made to appear, when boracic acid exists in small quantity in minerals, so as to afford a sure indication of its presence. That such a method is as yet a desideratum, will be obvious from the following observation, made by one of our first authorities on this subject. Berzelius observes, while speaking of boracic acid, " I have not hitherto succeeded in my attempts to discover a test for this acid by the blowpipe,—a thing much wanted, since, as well as the fluoric, it often occurs in minerals in very small proportion, and frequently escapes detection in analyses made in the moist way [*]."

When powdered boracite is moistened, and a particle of it is exposed on platinum-wire to the flame of the blowpipe, the characteristic green colour appears. Datolite, as well as the Humboldtite of Salisbury-Craig, gives no green tint to the flame when treated alone before the blowpipe; but if previously moistened by sulphuric acid, the green becomes very distinct,—a fact noticed in general terms by M. Pfaff in his Analytical Chemistry. Boracic acid has been detected in several varieties of tourmaline. Thus Arfwedson found about one per cent. of it in the blue tourmaline of Utön; M. Gruner discovered nine per cent. in a variety from Greenland; and, still more recently, Prof. Gmelin has detected the same acid in several other varieties of this mineral. When tourmaline is heated before the blowpipe, either alone or moistened with sulphuric acid, no trace of green appears; so that, if boracic acid is present, it cannot be detected by such means. To try if its presence could be discovered by other methods, I had recourse to the fluxes that have been recommended for lithia. The bifluate of potash, and the mixture of sulphate of ammonia and fluate of lime, gave no indication of boracic acid; but I succeeded completely with the flux which is composed of one part of fluate of lime and four and a half of the bisulphate of potash. About equal parts of this flux and powdered tourmaline are mixed together on the palm of the hand, being at the same time formed into a paste by a little moisture. A small particle of the mixture is then taken up on platinum-wire, and exposed to the blowpipe-flame, not at its apex, but somewhat nearer the wick than the point of the blue flame. Fusion takes place, and at the moment it does so, the portion of the flame beyond the assay is tinged of a pure green colour.

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[*] Berzelius on the Use of the Blowpipe, Children's translation, p. 130.

This effect is most distinct and unequivocal, but the operation requires some care. The green colour appears only for an instant, at the very commencement of fusion; and having once ceased, it cannot be made to appear again, however long the blast may be continued.

Through the kindness of my friends, Mr Allan and Mr Gregory, I have been supplied with a considerable variety of specimens of tourmaline and schörl, and all of them, without exception, give indications of boracic acid. The following is a list of those that have been examined:

Dark-blue Tourmaline, - -	from Massachusetts.
Green T. - - - - -	Do.
Black T. - - - - -	Brazils.
Black T. - - - - -	Abo, in Finland.
Black T. - - - - -	Finbo?
Black T. - - - - -	Arendal, in Norway.
Brownish-black T. - - -	Karingsbrycka, Sweden.
Black T. - - - - -	St Gotthard.
Black T. - - - - -	Cornwall.
Liver-brown T. - - - -	Do.
Liver-brown, fibrous and diverging, T.	Do.
Black T. - - - - -	Ross-shire.
Black T. - - - - -	Banffshire.
Black T. - - - - -	Aberdeenshire.
Black T. - - - - -	Germany.
P[illegible] - - - -	Penig, in Saxony.

From [illegible] of boracic acid in all these varieties, though found in such different parts of the world, it would seem to be an essential ingredient of tourmaline, as was rendered probable by the analyses of the chemists already referred to. The varieties from Aberdeenshire and Penig are specimens of common schörl, which occur in granite. The feldspar, in contact with the schörl, was carefully examined, but did not give the least indication of boracic acid.

As the process just described is of easy and rapid execution, and requires but a minute fragment of each specimen, I have not failed to examine a considerable number of minerals by this mode; and Mr Allan, with his usual liberality, has kindly supplied me from his cabinet with whatever was necessary for the purpose. The following list contains a few of the minerals so examined, in which no boracic acid could be discovered:

Pumice and Obsidian, from Lipari.
Pitchstone, from Arran and Meissen.
Greenstone, of Salisbury-Craig.
Basalt, of Arthur's Seat.
Common Hornblende, from Arendal.
Crystallised Hornblende, from Bohemia.
Augite, from Bohemia.
Common Garnet, from Greenland.
Bohemian Pyrope.
Pistacite, from Norway.
Feldspar.
Leucite.
Idocrase.
Zoizite.
Lava, origin unknown.

Axinite, on the contrary, though no boracic acid has hitherto been discovered in it, does certainly contain that substance; for, when treated by the flux, it yields precisely the same appearance as tourmaline. I first observed it in a specimen of my own, the locality of which is uncertain, but have since found it in crystallised axinite from Dauphiny and Cornwall, so that it is probably an essential ingredient of that mineral. The kind of rock from Cornwall, called Massive Axinite, does not contain boracic acid.

I possess a specimen of colophonite from Norway, supposed to be from Arendal, which likewise contains boracic acid. It appears, however, to be only an accidental ingredient; at least, two other varieties from Arendal, and a third from America, do not contain it.

It has of course been proved, that the green flame produced by the flux in tourmaline, axinite, and one variety of colophonite, was really occasioned by boracic acid. A specimen of Brazilian tourmaline, for example, was ignited with three times its weight of carbonate of soda; water was added, and the alkaline solution, after being neutralised by a slight excess of sulphuric acid, was evaporated to dryness. The dry mass was boiled in alcohol, and the solution, so formed, burned with a green flame. The same process was repeated with the colophonite and axinite with a similar result. I have not yet had leisure to determine how much boracic acid is contained in axinite, but, judging from the quantity of colour communicated to alcohol, it must be considerably less than in the Brazilian tourmaline.

Future observation must decide upon the value of the test here recommended. I know of no other substance but boracic acid that gives a green colour to the blowpipe-flame under the circumstances which have been described. A salt of copper tinges the flame green, but it does so without any flux at all. The mixture of fluate of lime and bisulphate of potash is applicable to saline as well as earthy minerals, since it causes the characteristic green colour, when fused with datolite and Humboldtite, equally well as with tourmaline and axinite. From the facility with which it acts on the latter, we may fairly presume that it would be equally efficacious in detecting the presence of boracic acid in any earthy mineral, if used in sufficient quantity. The proportion which seems best adapted for general use is two of the flux to one of the mineral, though in most cases much less of the former will suffice.

I cannot speak precisely as to the smallest quantity of the acid which may be detected by the blowpipe. According to the analyses of Arfwedson and Gmelin, some tourmalines contain only 1 per cent. of it; and hence we may infer that some of the varieties included in the foregoing list are similarly constituted. If this is the case, then the test must be a very delicate one; for I am satisfied, from the effect on the blowpipe-flame, that a less quantity of boracic acid could be detected, than exists in any of the tourmalines which have fallen under my notice.

With respect to the mode by which the flux acts, it is remarkable that the bifluate of potash alone does not cause the green colour to appear, not even with datolite. The pure fluate of lime, and even the bifluate of potash, is also ineffectual. It is hence probable that pure fluoric acid is useful, not only in assisting to sep [redacted] boracic acid from the substances with which it was cor mineral, but perhaps by forming the fluoric acid g

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Fig. 6.

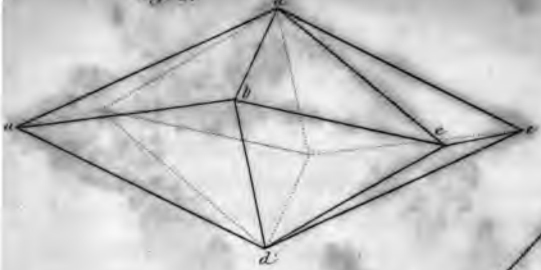


Fig. 1.

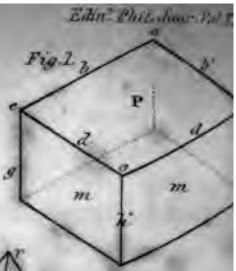


Fig. 7.

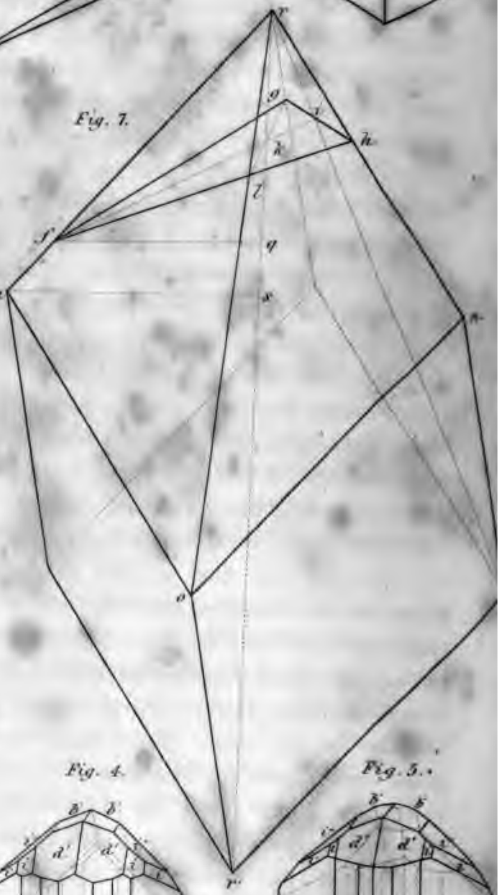
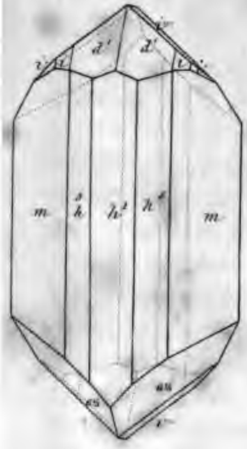


Fig. 2.



EUCLASE

Fig. 3.

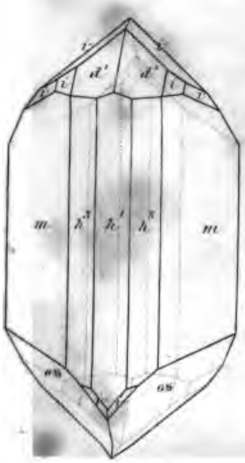


Fig. 4.

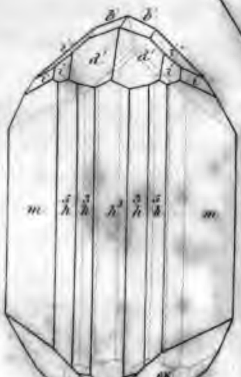
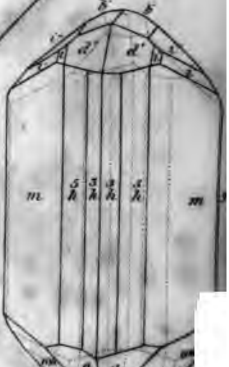


Fig. 5.



ART. XX.—*On Euclase.* By A. LEVY, Esq. A. M., &c. Communicated by the Author.

MR Heuland having lately added to his private collection some crystals of Euclase, uncommonly well defined, I have thought that their description might find room in your Journal, especially as the crystallographical characters of this substance have not hitherto been given with sufficient accuracy.

In preference to a right oblique-angled prism, the primitive form given by Haüy and Mr Phillips, I have adopted an oblique rhomboid prism, represented Plate VI. Fig. 1.*

All the secondary crystals derivable from the first of these two forms, are equally derivable from the second; and there is, undoubtedly, an advantage in point of simplicity, in not assuming more species of primitive forms than is really necessary. Not only Euclase, but all the substances for which a right oblique-angled prism has been chosen, as the primitive, may, for the same reasons, be made to derive from an oblique rhombic prism; and it is what Professor Mohs has already done, in referring them all to his Hemi-prismatic system. Cleavage, where it exists parallel to the faces of a right oblique-angled prism, cannot be made an objection against assuming an oblique rhombic prism as the primitive, when the numerous cases in which cleavages are found in directions different from those of the primitive planes are remembered, and when it is considered, that the faces of the right oblique-angled prism, which would have been used as the primitive, may always be made to correspond to some very simple modifications of the oblique rhombic prism.

In the present case, the only cleavages I have been able to observe, are parallel to the modifications h^1 and g^1 of the primitive form I have chosen, corresponding to the faces P and T of Mr Phillips. The cleavage parallel to his face *m*, which he has also observed, I have not been able to obtain; and, in consequence, the determination of the base of P, Fig. 1., has not been influenced by the direction of this cleavage.

The faces *m*, I have naturally chosen for the lateral planes of the primitive form, because they are always brilliant, and free from striæ, whilst it is just the reverse with all the other planes in the same direction; and I have determined the base by assuming, that the faces b, marked b', are the result of a decrement by one row upon the edges b of the primitive. This supposition gives simpler signs for the rest of the modifications, than several others I have tried. I have found the incidence of m on m 114°.50′, that of b' on m 91°.35′, and that of the two faces b' 143°.50′. By means of these data, and the supposition mentioned above, I have calculated the dimensions of the primitive; and from the parallelism of edges, where it was sufficient, or from observed incidences where it was not, I have calculated the other modifications.

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* This Plate will be given in next Number of the Journal.

Fig. 2. represents a crystal of a pale green, in which the faces $i'''' = (b^3\, d^{\frac{1}{2}}\, g^1)$ are very dull, and, consequently, this sign is only given as an approximation.

Fig. 3. represents a very well defined crystal, of a still paler green, and especially remarkable, by shewing both *summits.*

Fig. 4. represents a remarkably well defined crystal, nearly white.

Fig. 5. is the crystal which belonged to the Marquis de Dree's collection, and which has been mentioned in almost every treatise on mineralogy. The small triangular faces i' are dull, and I could not determine them either by parallelism of edges, or by measurements.

Dimensions of the Primitive Form and Table of the Modifications.

$m, m = 114°.50'$. $P, m = 118°.46'$. $b : h :: 1 : .5233$.

Plane angle of the base, $= 104°.12'$.

Plane angle of the later faces, $= 110°.32'$.

Modification	g^1	$m, g^1 = 122°.35'$	$p, g^1 = 90°$
Mod.	h^1	$m, h^1 = 147°.25'$	$g^1, h^1 = 90°$
Mod.	h^3	$m, h^3 = 165°.\ 8'$	$g^1, h^3 = 107°.43'$
Mod.	h^5	$m, h^5 = 170°.30'$	$g^1, h^5 = 113°.20'$
Mod.	b^1	$m, b^1 = 91°.35'$	$b^1, b^1 = 143°.50'$
Mod.	[illegible]	$m, b^{\frac{1}{3}} = 139°.44'$	$b^{\frac{1}{3}}, b^{\frac{1}{3}} = 105°.58'$
Mod.	[illegible]	$m, d^1 = 138°.23'$	$d^1, d^1 = 156°.10'$
Mod.	[illegible]	$m, a_2 = 131°.38'$	$a_2, a_2 = 151°.47'$
Mod.	[illegible]	$m, a_4 = 154°.32'$	$a_4, a_4 = 130°.15'$

Mod. $i = (d^1\, b^{\frac{1}{3}}\, g^{\frac{1}{2}})$ $m, i = 143°.58'$ $i, i = 134°.18'$
Mod. $i' = (d^{\frac{1}{3}}\, b^{\frac{1}{3}}\, g^{\frac{1}{2}})$ $m, i' = 147°.24'$ $i', i' = 99°.44'$
Mod. $i'' = (b^1\, d^{\frac{1}{3}}\, g^{\frac{1}{2}})$ $m, i'' = 99°.53'$ $i'', i'' = 113°.42'$
Mod. $i''' = (b^3\, b^{\frac{1}{2}}\, h^2)$ $m, i''' = 153°$ $i''', i''' = 122°$
Mod. $i'''' = (b^3\, d^{\frac{1}{2}}\, g^1)$ $m, i'''' = 116°$ $i'''', i'''' = 105°.20'$

Art. XXI.—*On the modes of Notation of Weiss, Mohs, and Haüy.* By A. Levy, Esq. M. A. &c. Communicated by the Author.

In the number of the Edinburgh Philosophical Journal for January 1825, I have given general formulæ to determine the law of decrement by which a Rhomboid, the incidence of the faces of which is known, may be supposed to be derived from another rhomboid, whose angle is also known, and which is considered as the primitive form with respect to the first. I have also begun to explain other formulæ relative to a particular case of the dodecaedrons, which are derivable from a rhomboid. Instead of proceeding now with the successive examination of the different decrements which may produce dodecaedrons, I shall consider at once the most general case, and deduce, afterwards, from it the particular ones.

Let *dd'*, Plate VI. Fig. 6., be a dodecaedron, derived by an intermediary decrement from the rhomboid *rr'*, Fig. 7. Let the axis of the rhomboid and dodecaedron be parallel, and the principal section *r o r'* of the first be parallel to the section *d b d'* of the second; then the plane *add'*, Fig. 6., will be parallel to the plane *m r r'*, Fig. 7. Let the plane *fgh*, Fig. 7., be parallel to one of the faces *a d b* of the dodecaedron; if we suppose the edge of the rhomboid to be one, and the lines *f r*, *h r*, *g r*, to be respectively $\frac{1}{x}$, $\frac{1}{y}$, $\frac{1}{z}$, the crystallographical sign of the dodecaedron would be $(b^{\frac{1}{x}}\, b^{\frac{1}{y}}\, b^{\frac{1}{z}})$; and the problem to be resolved, is to determine the indices $\frac{1}{x}$, $\frac{1}{y}$, $\frac{1}{z}$, or at least the ratios of the two last to the first, when the incidences of the faces of the dodecaedron are known. Not to repeat too often the crystallographical sign of the dodecaedron, I shall represent the faces by the letter *i*; the angle of two faces, such as *abd*, *bdc*, meeting in an edge, in the same direction as one of the oblique diagonals of the rhomboid, will be represented by $(i:i)$; that of two faces, such as *bdc*, *dce*, meeting in an edge situated in the same direction as one of the superior edges of the rhomboid, will be represented by $(i\,.\,i)$; and, finally, $(i\,,\,i)$ will designate the incidence of one of the faces, such as *abd*, upon the corresponding face *abd'* of the inferior pyramid. It is easy to demonstrate, that, in every dodecaedron derived from a rhomboid, there exists between these three angles the very simple relation expressed by the equation,

$$\sin \tfrac{1}{2}\,(i\,,\,i) = \cos \tfrac{1}{2}\,(i:i) + \cos \tfrac{1}{2}\,(i\,.\,i)$$

By means of which, two of these incidences being known, the third will be immediately found, especially as the value of any one of these three, deduced from the above equation, may, without difficulty, be transformed into another, to which logarithmic calculation may be applied.

Now, to resolve the proposed problem. The values of the angles $(i:i)$, $(i\,.\,i)$, $(i\,,\,i)$, should be expressed in terms of x, y, z, or rather the values of these last quantities in terms of the first. But the calculations necessary to be gone through to obtain them are very long; and the formulæ themselves are, besides, so complicated, as to be of very little use. Their comparison leads, however, to a simple result, which is sufficient to resolve most of the questions referring to dodecaedrons derived from a rhomboid, and which I shall demonstrate in a direct manner, without using the above mentioned formulæ.

Draw the oblique diagonals *r o*, *r p*, Fig. 7. and let them meet *f h*, *g h* in *l* and *i*. Join *f i*, *g l* meeting in *k*, and draw the axis *r k r'* of the rhomboid. It is obvious that the angle of the two planes *f g h*, *f k r'* is equal to $\tfrac{1}{2}\,(i\,.\,\mathrm{i})$, and the angle of the two planes *f g h*, *l k r'* is equal to $\tfrac{1}{2}\,(i:i)$; moreover the angle of the two planes *f k r'*, *l k r'* is equal to 60°. We shall have, therefore, by spherical trigonometry, in the triangular solid angle whose summit is at *k*, and formed by the three planes *f k l* or *f g h*, *f k r'*, *l k r'*, the two following equations:

$$\cos \tfrac{1}{2}\,(i\,.\,i)\ \sin fkl = \cos lkr' \,.\, \sin fkr' - \tfrac{1}{2} \sin lkr' \,.\, \cos fkr'$$
$$\cos \tfrac{1}{2}\,(i:i \text{ [illegible] } \cos fkr' \,.\, \sin lkr' - \tfrac{1}{2} \sin fkr' \,.\, \cos lkr'.$$

and dividing the first by the second,

$$\frac{\cos\frac{1}{2}\,(i\,.\,i)}{\cos\frac{1}{2}\,(i:i)}=\frac{2\,\text{tang}\,f\,k\,r'-\text{tang}\,l\,k\,r}{2\,\text{tang}\,l\,k\,r'-\text{tang}\,f\,k\,r'}.$$

We shall obtain, consequently, the value of the ratio of these two cosines, if we can get those of the tangents of the angles $f\,k\,r'$ and $l\,k\,r'$. It is even sufficient to determine the value of tang $f\,k\,r'$, for, in changing in it x into z, and z into x, we shall get the tangent of $g\,k\,r'$, and by changing the sign of this, the tangent of $l\,k\,r'$.

From f and m, let $f\,q$, $m\,s$, be drawn perpendicular upon $r\,r'$, let $r\,s=a$, and $m\,s=p$, then $r\,q=\frac{a}{x}$, $f\,q=\frac{p}{x}$, $r\,k=\frac{3\,a}{x+y+z}$, consequently $k\,q=\frac{a\,(y+z-2\,x)}{(x+y+z)\,x}$, and tang $f\,k\,r'=\frac{p}{a}\cdot\frac{x+y+z}{y+z-2\,x}$, and tang $l\,k\,r'=-\frac{p}{a}\cdot\frac{x+y+z}{y+x-2z}$.

These values being substituted in the expression gives,

$$\frac{\cos\frac{1}{2}\,(i\,.\,i)}{\cos\frac{1}{2}\,(i:i)}=\frac{y-z}{x-y}.$$

This formula will give at once a simple relation between the three unknown quantities x, y, z, when the two angles $(i\,.\,i)$, $(i:i)$ are known. It is also a test of the simplicity of the indices of the secondary planes, which we are considering; for if these indices, that is x, y, z, are always simple numbers, it necessarily follows that $\frac{y-z}{x-z}$, or its equal, the ratio of the cosines of half the two pyramidal angles of any dodecahedron derived from any rhomboid, is always a simple integral, or fractional number; a result the correctness of which I have had frequent opportunities to verify.

It is now easy to apply the preceding formula to the dodecaedrons which result from simple decrements, by assuming proper values for x, y, and z. Thus, by taking $x=o$, $y=1$ and $z=n$, the formula will correspond to the case of a dodecaedron produced by n rows in breadth on the superior edges of the rhomboid, the sign of which is b_n, and will become

$$\frac{\cos \frac{1}{2}\,(b^n \,.\, b^n)}{\cos \frac{1}{2}\,(b^n : b^n)} = n - 1.$$

By making in the same formula $x = 1$, $y = o$ and $z = -n$, it will correspond to the case of a dodecaedron produced by n rows in breadth on the inferior edges of the rhomboid, the sign of which is d^n, and will become

$$\frac{\cos \frac{1}{2}\,(d^n \,.\, d^n)}{\cos \frac{1}{2}\,(d^n : d^n)} = n.$$

Lastly, By supposing $x = -1$, $y = 1$, and $z = n$, it will correspond to the case of a dodecaedron produced by n rows in breadth on the lateral angles of the primitive, the sign of which is e_n, and will become

$$\frac{\cos \frac{1}{2}\,(e_n \,.\, e_n)}{\cos \frac{1}{2}\,(e_n : e_n)} = \frac{n - 1}{2}.$$

These three formulæ will immediately give the law of decrement by the simple subtraction of two logarithms, when two of the incidences of the faces of the dodecaedron will be known.

The first shews than when $n = 2$, the angle $(b^n \,.\, b^n) = (b_n : b^n)$, that is to say, that a decrement by two rows on the superior edges will produce dodecaedrons with isosceles triangular planes.

The second ormula makes the two angles $(d^n \,.\, d^n)$, $(d^n : d^n)$ equal, only when $n = 1$, in which case the result of the decrement is the lateral planes of a six-sided prism.

The third formula shews that when $n = 3$ the angle $(e_n \,.\, e_n) = (e_n : e_n)$, that is to say that a decrement by three rows on the lateral angles of a rhomboid will produce dodecaedrons with isosceles triangular planes.

Returning now to the general case, the origin of hypothetical primitive forms, and the reasons for which a dodecaedron resulting from an intermediary decrement upon the angles of the primitive rhomboid, is, and has always been found to result of a very simp[illegible]ement on the edges or angles of the hypothetical prim[illegible]y readily be discovered. For, it is obvious fro[illegible]ding formulæ, that if the dodecaedron, Fig. 6. be considered as deriving from a rhomboid, the superior edges of which should correspond to the lines *d a*, *d c*, &c., by a decrement on its superior edges, the law of that decrement would be expressed by $\frac{y-z}{x-y}+1=\frac{x-z}{x-y}$. It is equally evident that the same dodecaedron may be considered as the result of a decrement by $\frac{x-z}{y-z}$ rows in breadth on the superior edges of the rhomboid, whose superior edges should correspond to the lines *a b*, *a d*, &c.; or again, by $\frac{y+z-2x}{y-z}$ rows in breadth on the lateral angles of the rhomboid, the oblique diagonals of which should correspond to the lines *d a*, *d c*, &c.; or by $\frac{y+x-2z}{x-y}$ rows in breadth on the lateral angles of the rhomboid, the oblique diagonals of which should correspond to the lines *a b*, *a d*; or, lastly, by $\frac{y-z}{x-y}$ rows in breadth on the inferior edges of the rhomboid, the inferior edges of which should correspond to the lines *a b*, *b c*, *c d* of the dodecaedron. And since x, y, z are found to be generally simple numbers, it is clear from the expression we have just found for the laws of decrements on the hypothetical primitive forms, that they will also generally be very simple.

(To be continued.)

Art. XXII.—*On the Preservation of Zoological Specimens from the Depredations of Insects.* By Thomas S. Trail, M. D. F. R. S. E., &c. * Communicated by the Author.

THE difficulty of preserving zoological specimens from the depredations of insects, is a subject of regret and anxiety to every collector; and various methods have been proposed of accomplishing this desirable object. The compositions into which *arsenic* and *corrosive sublimate of mercury* enter, are well known to be very effectual, when properly applied; but, unless used with caution, they are apt to injure the natural pliancy of the skins, and they can scarcely be effectually employed to protect collections of insects. I have known these substances, even in the hands of the most expert, produce such *tenderness* of the skins impregnated with them, as to form a considerable obstacle to the *setting up* of the specimens. To render them effectual, too, they must be carefully applied to each specimen; by which the labour of collecting and preserving is materially increased.

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* The method of preserving zoological specimens recommended by Dr Traill, we have been in the practice of employing to great extent, and most successfully, in the Museum of the University, for a considerable time past.—Ed.

Of the method proposed, by M. Temminck, viz. the introduction of *tallow* into the cases containing zoological specimens, I am yet unable to speak from experience. It has been lately introduced into the Museum of our Royal Institution, where it will have a fair trial, although I confess that its *modus operandi* does not seem very obvious *.

Camphor has been long known as a preservative against the attacks of insects; yet I have known specimens of birds to suffer from moths, though inclosed in boxes in which camphor was present; and, to be efficacious, it ought to be used in considerable quantity.

Every substance which I have yet tried, seems to be inferior in efficacy and ease of application to the following,—the method of Mr William Gibson, preparer of objects of Natural History, residing in No. 16, London Road, Liverpool,—which I shall transcribe from his own communication to me.

" I have found," says he, " that nothing destroys insects so effectually as *rectified oil of turpentine*, and my method of using it is as follows: I put the turpentine in a bladder, the mouth of which is firmly tied with a waxed string; and nothing more is necessary than to place the bladder, thus prepared, in the box with the birds, or to tie it to the pedestal on which the birds are perched, in a case. If there be any maggots on the birds,

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* I did not [illegible] als, that tallow placed in cases containing zoological specimens d[illegible]

I have invariably found, that they will soon be dislodged from the feathers, fall to the bottom of the case, and die in the course of two days. I have also made the experiment of introducing the common house-fly, the large blue-bottle-fly, and moths, into a case of birds so defended, through a small hole in the bottom of the case. The moment the flies enter the box, they begin to vomit a whitish, glutinous matter, they are much agitated, and the largest of them died in seven minutes. I have, in like manner, repeatedly introduced active American cockroaches, and these strong insects soon became uneasy, often rubbed their sides with their hind feet, and died in about an hour and a half. I next got a bird-skin full of living maggots, and placed it in my defended case; in about three hours they were seen *coming out* in all directions, and fell to the bottom of the case, where they died. For large cases of birds, a pig's or a sheep's bladder is sufficient; for middle sized cases, a lamb's or a rabbit's bladder will do; and for small ones, we may use a rat's bladder. The turpentine evidently penetrates through the bladder, as it fills the case with its strong smell."

The powerful anthelmintic effect of oil of turpentine, corroborates Mr Gibson's account of its poisonous quality to the larvæ of insects; and its instantaneously killing perfect insects, must be familiar to the entomologist. I may here remark, that I have found the common receipt of dipping the pin, with which the insect is to be transfixed, in aquafortis, is by no means so speedy a method of putting an end to its sufferings, as applying a single drop of turpentine to the corselet. Though disappointed in the use of the pin dipt in acid, I never found the largest insects, *Libellulæ*, *Scarabæi*, *Blattæ*, or *Scolopendræ*, that could, for a moment, resist the application of oil of turpentine*. I ought to add, however, that my entomological pursuits have been few; for the difficulty of speedily killing insects, without injuring the specimen, early gave me a distaste to that branch of Natural History.

The difficulty of destroying the minute white *acari*, that infest the hairs of specimens in collections, is well known. On the neck of a large specimen of *Phoca leonina* (Linn.), in our Museum, I lately observed innumerable *acari.* I directed the skin to be carefully and repeatedly washed with a strong solution of corrosive sublimate in spirit, seemingly without much effect. Some of them even crawled among the hairs while still wet with this solution; but on brushing the part infested by these vermin with oil of turpentine they speedily disappeared.

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* I have seen several coleopterous insects swimming about for some time in strong spirits; but immersion in oil of turpentine, uniformly, was speedily fatal.

Though similar facts are not unknown to naturalists, it is singular that this liquid has not been hitherto applied to preserve dried zoological specimens from insects; and Natural History will thereafter derive much benefit from this simple and effectual process. As far as I can judge, this method promises, from its cheapness, and easy application, to be very useful, not only in collections of Natural History exposed to public view, but will materially abridge the labour, and save the precious time, of the scientific traveller in preserving his collections. It will also, I doubt not, prove an acceptable boon to furriers and other dealers in peltry [*].

Art. XXIII.—*Notice of Zircon found in the primitive Island of Scalpay, on the East Coast of Harris.* By William Nicol, Esq. Lecturer on Natural Philosophy. Communicated by the Author [†].

The distribution of simple minerals in the various rock-formations of Scotland, has hitherto engaged comparatively little of the attention of mineralogists, geognosy being still, with the majority of naturalists, the favourite pursuit. However much we may feel disposed to exult in the striking discoveries and grand views of the mineral kingdom, opened up to us by the sagacity, skill, and enterprise of geologists; still we cannot help expressing our regret that the minuter, although equally beautiful, displays of the subterranean world, as exhibited to the attention of the mineralogist in the various forms, structures, and arrangements of simple minerals, should have hitherto been so little regarded by the mineralogists of this country. It cannot, with any justice, be said, that the mountains, and hills, and cliffs of Scotland, are barren of simple minerals; for the small portion of attention bestowed on their investigation, has proved, not only that this is not the case, but, on the contrary, that our mineral formations promise, to the skilful and active explorer, as abundant a return as these of any other country in Europe. Let, then, some of our mineralogists devote themselves to that delightful occupation, the tracing out of simple minerals in our strata, beds, and veins, and ere long the mountains of Scotland will become as distinguished in mineralogy for the beauty and variety of their simple minerals, as they now are for the numberless important geognostical relations which they exhibit.

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[*] Oil of [illegible]d in Ceylon in India for destroying bugs, a practice also to [illegible] adoption in this country.—Ed.

[†] Read [illegible]n Natural History Society 17th Dec. 1825.

Already Professor Jameson has enumerated, in his mineralogical writings, the following gems as natives of Scotland, viz. Precious Beryl, Schorlite, Cinnamon-Stone, Zircon, Topaz, Garnet, and Amethyst *. Of these gems the schorlite and zircon are the rarest.

During a tour through the Hebrides last summer, I visited the lone and rugged regions of Harris, whose geognosy, like that of the whole of the dreary island range, named Long Island, we may say is almost unknown; for the vague and rambling notices published, contain little information, and that little not deserving of commendation, on the score either of accuracy or consistency.

In a small island named Scalpay, situated on the east coast of Harris, I met with crystals of one of the rarer of the gems,—the Zircon.

These were imbedded in a mass of chlorite, subordinate to gneiss, and in some parts of the rock were very numerous. The crystals are brown, inclining more or less to red. The following crystallizations were met with.

1. Rectangular four-sided prism, sometimes slightly truncated on the lateral edges, and generally acutely acuminated on each extremity by eight planes, of which two and two meet under very obtuse angles, and are set on the lateral planes of the

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* *Mineralogy of the Scottish Isles*, 2 vols. 4to.; *System of Mineralogy*, 3 vols. 8vo.; *Memoirs of the Wernerian Natural History Society*, vol. i. p. 445; *Manual of Mineralogy*; *Annals of Philosophy*; and *Edinburgh Philosophical Journal*.

prism; and these again obtusely acuminated by four planes, which are set on the obtuse edges of the first acumination. This second acumination appears sometimes to terminate in a line, when two opposite planes are much larger than the others.

2. Rectangular four-sided prism, acuminated by four planes, which are set on the lateral planes, and the angles formed by the meeting of the acuminating and lateral planes bevelled.

3. In some crystals the acumination on one extremity is simply the acute eight-sided pyramid, while, on the opposite, it is the double acumination already mentioned.

In some specimens, the length of the crystals is three or four times greater than the breadth, and in others the crystal is so short, that the acuminating planes of the opposite ends meet in the lateral edges. The surface of the crystals is smooth and shining, and they range from transparent to feebly translucent. They are generally small, many of them not exceeding in size the head of the smallest pin. The largest I detached is about $\frac{2}{10}$ths of an inch in length.

Specific gravity 4.409, Dr Turner.

It is right to add, that the chlorite containing the zircon is associated with magnetic iron-ore, talc-slate, and serpentine.

Art. XXIV.—*On the Effects of Temperature on the Intensity of Magnetic Forces; and on the Diurnal Variation of the Terrestrial Magnetic Intensity.* By S. H. Christie, Esq. M. A. of the Royal Military Academy.

In the last Number of this Journal, we laid before our readers an interesting extract from the memoir of Mr Christie on Magnetism, &c. not then published. This important memoir having just appeared in the *Philosophical Transactions*, Part I. for 1825, we shall now state some of the facts and views which it contains. It commences as follows.

"In the paper on the diurnal deviations of the horizontal needle when under the influence of magnets, which the President did m[illegible]ur to present, I stated that these deviations were partly [illegible]f changes that took place in the temperature of th[illegible]d that although the conclusions which

I drew from the observations respecting the increase and decrease of the terrestrial magnetic forces during the day would not be materially affected, it was my intention to undertake a series of experiments for the purpose of determining the precise effects of changes of temperature in the magnets, so as to be able to free the observations entirely from such effects.

" These experiments were immediately made; but I was induced, from some effects which I observed, to carry them to a greater extent, in the scale of temperature, than was necessary for the object which I had at first in view. In consequence of this, and the length of the calculations into which I have been obliged to enter, the accomplishment of my purpose was delayed for a considerable time, and continued indisposition has since prevented me, until now, completing the arrangement of the tables of results.

" In the present paper, I propose to detail the experiments which I made, in order to determine the effect of changes of temperature on the forces of the magnets, to the extent to which I observed their temperature to vary, during my observations on the diurnal changes in the direction of the needle, when under their influence; to apply the results which I obtained to the correction of the observations themselves, thereby accounting for the apparent anomalies noticed by Mr Barlow and myself, in the observations made in-doors and in the open air; and, by means of these corrected observations, to point out the diurnal variations in the terrestrial magnetic intensity."

Having found it impracticable to determine purely from observation the portion of the arc of deviation due to the changes which he noticed in the temperature of the magnets, Mr Christie was, therefore, under the necessity of having recourse to theory; and he adopted the simplest, and that which is most generally received, viz. that the forces which two magnets exert upon one another may be referred to two centres or poles in each, near their respective ends; and that for either pole in one of the magnets, one pole of the other magnet is urged towards it, and the other from it, by forces varying inversely as the squares of their respective distances from that pole.

After this statement, he proceeds to explain and exemplify the application of the theory to the investigation detailed in the paper; and then, describing the compass and magnets made use of (the verbal description being illustrated by an engraving), he gives the subjoined account of the mode of experimenting adopted.

" A meridian line being drawn on a firm table, standing on a stone floor, the compass was accurately adjusted on it, so that the needle pointed to zero on the graduated circle. The magnets were fixed at the bottoms of earthen pans, secured in such a way to rectangular pieces of board that their positions could not be accidentally changed, and projecting from these boards were small pieces of brass, on each of which a line was drawn, to indicate the position of the axis of the magnet; the horizontal distance of the edge of each of the projections nearest to the needle from the corresponding end of the magnet within the pan, was exactly three inches; I could, therefore, in any instance, determine very accurately the distance of the centre of the magnet from that of the needle. The pans were placed on the table, so that the indexes on the pieces of brass coincided with the meridian line. Water was now poured into the pans, and the temperature of the magnets was varied by varying the temperature of the water. The temperature of each magnet was ascertained by a thermometer placed in the water, with its bulb resting on that pole of the magnet which was nearest to the centre of the needle. In my first observations I, however, made use of only one thermometer, which was moved during them from one magnet to the other."

" The observations contained in the tables were made thus: I first noted the time, and then the temperature of the north magnet; after which I placed the thermometer on the pole of the south magnet. I next observed the westerly point, at which the needle was held *in æquilibrio* by the terrestrial forces and those of the magnets, slightly agitating the needle, that it might the more readily assume the true position; from this it was led, by means of a very small and weak magnet, held on the outside of the compass-box, towards the easterly point of equilibrium, which was observed in the same manner; and from [illegible] it was led in the same way towards the southerly poin[illegible]se observations of the points of equilibrium, th[illegible]f the south magnet being observed, the time at which the observations concluded was noted. The temperature of the water in the pans was now increased or diminished, according to circumstances, by the addition of other water, and the pans covered over, to prevent any rapid changes of temperature during the observations. After allowing a short time for the magnets to acquire the temperature of the water, the observations were repeated. The scale made use of for the temperature was in all cases that of Fahrenheit."

From the results of the observations given in the tables described in the paragraph last quoted, we extract the following:

" *Table of the Magnetic Intensities corresponding to different Temperatures of the Magnets.* *6th June* 1823.

Mean Temp. of the Magnets.	Diff. of Temp. in successive observations.	Magnetic Intensity or Values of $\frac{F}{M}$.	Variation of $\frac{F}{M}$ for 1° Fahr. or $\Delta . \frac{F}{M}$.
62°.05		212.5620	
59.05	− 3.00	212.9423	0.1268
77.65	+ 18.60	210.6228	0.1247
74.00	− 3.65	210.9892	0.1004
70.65	− 3.35	211.4178	0.1279
67.15	− 3.50	211.8353	0.1193
63.80	− 3.35	212.2167	0.1138
62.05	− 1.75	212.4640	0.1413

Some anomalies observed by Mr Barlow between the daily changes in the direction of a needle, when placed in the house and when in the open air *, which Mr Christie also noticed, and stated, in a former paper, his opinion that they had arisen from the difference in the changes of temperature in the magnets in the two situations, are next investigated in the memoir before us; observations on the temperature of the magnets having been made in the open air, corresponding to those made indoors.

We select the subjoined table from among the results of this branch of Mr Christie's inquiry.

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* These anomalies are described by Mr Barlow in his paper on the daily variation of the horizontal and dipping needles under a reduced directive power.

Mean Temp. of the Magnets.	Diff. of Temp. in successive observations.	Magnetic Intensity or Value of $\frac{F}{M}$.	Variation of $\frac{F}{M}$ for 1° Fahr. or $\Delta \cdot \frac{F}{M}$.
49°.30		224.0981	
60.25	+ 10.95	222.8171	0.1179
68.25	+ 8.00	221.7046	0.1391
74.60	+ 6.35	220.7198	0.1551
61.75	—12.85	222.3967	0.1305
73.80	+ 12.05	220.8778	0.1260
55.58		222.6462	
66.00	+ 10.42	221.2655	0.1315
73.60	+ 7.60	220.1532	0.1461
56.90	—16.70	222.5145	0.1314

A double series of observations on the diurnal changes in the positions of the points of equilibrium at which a magnetic needle was retained by the joint action of terrestrial magnetism and of two bar magnets, having their axes horizontal and in the magnetic meridian, and their centres at the distance 21·21 inches from the centre of the needle, afford by correction and calculation the following

Tables of the Mean Terrestrial Magnetic Intensities at different Hours during the Day.

1. From observations made within doors.

Time of Observation.	*Mean of the Observations of May 22, 23, 24, 25, 26.*		*Mean of the Observations of May 27, 28, 29, 30, 31.*		*Mean of the two Sets.*
	Azimuth of the points of Equilibrium.	Terrestrial Magnetic Intensity.	Azimuth of the points of Equilibrium.	Terrestrial Magnetic Intensity.	Terrestrial Magnetic Intensity.
6h 00m	81° 27.3′	1.00175	81° 56.9′	1.00170	1.00173
7 30	82 19.9	1.00100	82 27.4	1.00128	1.00114
9 00	83 13.9	1.00031	83 33.6	1.00046	1.00039
10 30	83 40.5	1.00000	84 16.2	1.00000	1.00000
Noon.	82 22.8	1.00096	83 40.3	1.00038	1.00067
1 30	81 43.5	1.00151	82 39.5	1.00112	1.00132
3 00	81 29.1	1.00173	81 57.2	1.00170	1.00172
4 30	81 11.5	1.00199	82 10.8	1.00151	1.00175
6 00	81 17.7	1.00190	81 41.7	1.00192	1.00191
7 30	81 00.9	1.00216	81 20.5	1.00224	1.00220
9 30	80 52.6	1.00229	81 14.5	1.00233	1.00231
11 20			81 19.7	1.00225	1.00225

“ Fro [illegible] tained here, it appears that the terrestrial magnetic [illegible] the least between 10 and 11 o'clock in the morning, the time, nearly, when the sun was on the magnetic meridian; that it increased from this time until between 9 and 10 o'clock in the evening; after which it decreased, and continued decreasing during the morning until the time of the minimum."

2. From observations made in the open air.

Time of Observation.	*Mean of the Observations of June* 20, 21, 22.	
	Azimuth of the point of Equilibrium.	Terrestrial Magnetic Intensity.
6^{h} 00^{m}	79° 30.0	1.00112
7 30	79 51.7	1.00061
9 00	80 24.7	1.00028
10 30	80 42.2	1.00000
Noon.	80 32.7	1.00015
1 30	79 23.0	1.00134
3 00	78 53.2	1.00188
4 30	78 34.8	1.00223
6 00	78 20.3	1.00251
7 30	78 26.5	1.00239
9 00	78 42.3	1.00209

"From these it appears, that the minimum intensity happened nearly at the time the sun passed the magnetic meridian, and rather later than in May, which was also the case with the time of the sun's passage over the meridian. The intensity increased until about 6 o'clock in the afternoon, after which time it appears to have decreased during the evening, and to have been decreasing from an early hour in the morning.

"The general agreement of these intensities with those deduced from the observations made in-doors, is as near as could be expected, considering that an interval of twenty days had elapsed between the two sets of observations. From this, and the agreement in the manner in which the westerly and easterly points of equilibrium approach and recede from the north in the two cases, which I have before pointed out, we may conclude, that there is nothing anomalous in the action which takes place on the needle under the different circumstances of its being placed in-doors or in the open air; and that the apparent anomaly in the directions of the needle in the two cases, which was observed by Mr Barlow and myself, arose from the cause which I have assigned for it in my former paper; namely, the difference in the changes of temperature in the magnets when in-doors and when in the open air.

"The diurnal changes in the terrestrial magnetic intensity have been determined by Professor Hansteen, by means of the vibrations of a needle delicately suspended. From these observations it appears, that, in general, the time of minimum intensity was between 10 and 11 o'clock in the morning; that the maximum happened between 4 and 7 for the month of May 1820, and about 7 o'clock in the evening for the month of June. The intensity which, in these observations, is taken as unity, is that deduced from an observation made during an aurora borealis; but, for the purpose of comparison, I have, for the months of May and June, taken the intensity deduced from his observations at $10^h\ 30^m$ in the morning as unity, reduced the intensities, which he gives for other times in the day, to this standard, and placed them in the following table, with the corresponding intensities deduced from my own observations.

Intensity deduced from Hansteen's Observations in 1820.			*Intensity deduced from the preceding Observations in 1823.*		
Time.	May.	June.	Time.	May.	June.
$8^h\ 00^m$ A. M.	1.00034	1.00010	$7^h\ 30^m$ A. M.	1.00114	1.00061
10 30	1.00000	1.00000	10 30	1.00000	1.00000
4 00 P. M.	1.00299	1.00251	4 30 P. M.	1.00175	1.00223
7 00	1.00294	1.00302	7 30	1.00220	1.00239
10 30	1.00191	1.00267	9 30	1.00231	1.00209

"The principal difference to be observed in the nature of the changes of intensity during the day, in the two cases, is, that, from my observations, the intensity appears to decrease more rapidly in the morning, and increase more slowly in the afternoon, than it does from those of Professor Hansteen; but the general character of these changes is as nearly the same as we can expect from methods so different, at different times, and at places where both the variation and dip of the needle are different. My object, however, was to point out what might be deduced fr [illegible] eries of such observations as I have detailed, rather th [illegible] the results deduced from them with those obtained by others, for which purpose it would have been necessary to have continued them for a greater length of time.

"We have seen, that with the magnets I made use of, their intensity being nearly 218 M, at the temperature 60°, a change in their temperature of 1° would cause a change of intensity of 0.123 M; or taking the intensity of the magnets 1, for each degree of increase in temperature we should have a decrease of intensity of 0.000564. Now, if the same, or nearly the same, take place with all magnets, it is evidently necessary, in all cases where the terrestrial magnetic intensity is to be deduced from the vibrations of a needle, that great care should be taken to make the observations at the same temperature; or, the precise effect of change of temperature having been previously ascertained, to correct the observations according to the difference of the temperatures at which they were made. I am not aware that any one has yet attempted to make such a correction; but it is manifest from the experiments I have described, that it is indispensable, in order to deduce correct results from the times of vibration of a needle in different parts of the earth, where the temperatures at which the observations are made are almost necessarily different, that these temperatures should be registered, and the times of vibration reduced to a standard of temperature. It appears to me, that the effects will be the most sensible in large and powerful needles; and consequently, in making use of such, the reduction for a variation of temperature will be most necessary. There would be no difficulty in this reduction, if we could give, in terms of the intensity of any magnet, the increment or decrement of intensity corresponding to a certain decrement or increment of temperature at all temperatures. To determine this accurately would, however, require a great variety of experiments to be made with magnets of very different intensities; but, as I have not made these, I must content myself for the present with pointing out some of the facts which I have ascertained from more extended experiments than those I have already given, reserving the detail of these experiments for another opportunity, should they be deemed of sufficient interest.

"These experiments were made with a balance of torsion, the needle being suspended by a brass-wire $\frac{1}{450}$th inch in diameter. By them I ascertained the following facts.

" 1. Commencing with a temperature — 3° Fahrenheit, up to a temperature of 127°, as the temperature of the magnets increased, their intensity decreased. Owing to the almost total absence of snow during the winter, I was unable to reduce lower the temperature of the large magnets which I made use of; but, from an experiment I made at the Royal Institution, in conjunction with Mr Faraday, in which a small magnet, enveloped in lint well moistened with sulphuret of carbon, was placed on the edges of a basin containing sulphuric acid, under the receiver of an air-pump, I found that the intensity of the magnet increased to the lowest point to which the temperature was reduced, and that the intensity decreased on the admission of air into the receiver, and consequent increase of temperature in the magnet. This is in direct contradiction to the notion which has been entertained of destroying the magnetism of the needle by the application of intense cold.

" 2. With a certain increment of temperature, the decrement of intensity is not constant at all temperatures, but increases as the temperature increases.

" 3. From a temperature of about 80°, the intensity decreases very rapidly as the temperature increases: so that, if up to this temperature, the differences of the decrements are nearly constant, to ascertain which requires a precision in the experiments that perhaps their nature does not admit of, beyond this temperature the differences of the decrements also increase.

" 4. Beyond the temperature of 100°, a portion of the power of the magnet is permanently destroyed.

" 5. On a change of temperature, the most considerable portion of the effect on the intensity of the magnet, is produced instantaneously; shewing that the magnetic power resides on or very near the surface. This is more particularly observable when the temperature of the magnet is increased, little change of intensity taking place after the first effect is produced; on the contrary, when the temperature of the magnet is diminished, although ne[illegible]hole effect is produced instantly, yet the magnet ap[illegible]ue to gain a small power for some time.

" 6. The effects produced on unpolarised iron by changes of temperature, are directly the reverse of those produced on a magnet; an increase of temperature causing an increase in the magnetic power of the iron, the limits between which I observed being 50° and 100°. That the effect on iron of an increase of temperature should be the reverse of that produced on a magnet, is, I think, a strong argument against the hypothesis, that the action of iron upon the needle arises from the *polarity* which is communicated to it from the earth.

" It may be objected to the method which I have adopted for determining the diurnal changes in the terrestrial magnetic intensity, that, after the observations have been made, they require a correction for temperature, which can only be determined by experiments previously made on the magnets and needle employed. The same objection may, however, be made against the method of determining the intensity by the vibrations of a needle. As such a correction has not, in the latter case, been hitherto applied, the results which have been obtained relative either to the diurnal changes of intensity, or the intensities in different parts of the earth, by means of observations on the vibrations of a needle, will be so far incorrect as the needle may happen to have been affected by differences in the temperature. The method I have described, however, possesses advantages over the other: a very considerable one is, that, whatever effects are produced, may easily be observed with considerable precision, the time required for each observation being not more than five minutes; another is, that, the magnets being immersed in water, as far as regards them, we may command the temperature at which the observations are to be made, and thus limit the correction for temperature to a very small quantity; and it possesses another decided advantage, that whatever are the effects produced on the needle by atmospheric changes, they are, by means of it, rendered immediately visible, and can be observed as they occur *."

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* A series of experiments on the Effects of Temperature on Magnetism, by Dr Kupfer, Professor of Natural Philosophy and Chemistry at Kasan, has appeared in the 6th volume of Karsten's Archiv für Naturliche.—EDIT.

ART. XXV.—*List of Rare Plants which have Flowered in the Royal Botanic Garden, Edinburgh, during the last three months.* Communicated by Professor GRAHAM.

Dec. 6. 1825.

Acacia Lophantha.
Amaryllis aurea.
Banksia ericifolia.
Bignonia grandifolia.
Camellia oleifera.
Columnea hirsuta.
Cunonia capensis.
Cyathodes abietina.
Epidendrum umbellatum.
Eranthemum variabile.
Fuchsia arborescens.

We have plants raised from seeds under this name, which can be readily distinguished, by their remarkable glaucous appearance, from those which have flowered; but as this seems the only distinction, it is not unlikely they may lose it when they grow older. In Bot. Mag. fol. 2620. a hope is expressed that this species may be found hardy enough to bear our winters without protection; but it and the F. excorticata were among the first which were cut up by the cold this winter, in a plot of the different species of the genus in the Botanic Garden. It forms, however, a very handsome greenhouse shrub.

Gonolobus diadematus.
Hemimeris peduncularis.
Ixora arborescens.
Jasminum paniculatum.
Lechenaultia formosa.
Liparia vestita.
Lobelia g
Ornithidi
Rhus ver

Thunbergia capensis.
——— coccinea.

This fine stove plant was received from the Calcutta Garden under this name in 1823; but we have no history of the species.

I would suggest the following as its essential character and description:

T. coccinea.—Corolla subringenti, limbo arcte reflexo; racemis interruptis, terminalibus, secundis; foliis angulatis, hastatis; caule volubili.

DESCRIPTION.—*Root* throwing up many stems. *Stems* branching from the bottom; branches axillary, opposite, slightly swollen at the joints, and climbing to a great height, twining from left to right, green, smooth. *Leaves* opposite, petioled, pale green, lighter on the back, smooth, angular, especially towards the base, hastate, acuminate, 5 larger 2 or 4 smaller nerves; nerves prominent, especially on the back, and there reticulated. *Petiole* half the length of the leaf, channelled above. *Flowering branches* long (1–2 feet), axillary, leafy; the leaves opposite, and resembling those on the stem, but smaller, and gradually diminishing in size, and becoming cordate towards the flowers. *Racemes* long, terminal, interrupted, secund. *Pedicels* two-thirds of the length of the flowers, two, three, or more, arising from the axilla of each leaf or bractea, stout, and swelling slightly upwards; bracteas often awanting towards the extremity of the raceme. *Outer calyx* as long as the tube of the corolla, almond-shaped, reddish-brown, bursting along one edge, and falling after the corolla in one piece; veins inconspicuous, numerous, parallel. *Inner calyx* cup-shaped, nearly entire. *Co-*

rolla subringent; *tube* pale red, secreting a large quantity of honey, dilated at the base, mouth compressed, oblique; *limb* bright red, 5-cleft, segments obtuse, closely reflexed upon the tube and outer calyx. *Stamens* included, 4 fertile, didynamous, 1 abortive, varying in length, and adnate towards its base with the tube of the corolla behind the style; *filaments* inserted into the corolla where it begins to dilate at the base, stout, red, compressed; *anthers* large, yellow, ciliated, mucronate at the base, mucros red, smooth. *Germen* yellow, urceolate, beaked, beak green. *Style* rather slender, longer than the filaments, white, compressed, bent to a right angle near the top; *stigma* white, cleft, sub-exserted.

This species, as well as some of those lately published, shews that the form of the corolla varies greatly, and seems to indicate the propriety of striking it out of the essential generic character. This species also shews that in the genus there is a want of uniformity of calyx.

Tulbagia alliacea.

Valisneria spiralis (fœm.)

This most desirable plant was introduced into the garden from the St Lawrence, in the neighbourhood of Sorell, 160 miles above Quebec, by the kindness of the Countess of Dalhousie.

ART. XXVI.—*Meteorological Observations made at Leith.*

By Messrs COLDSTREAM and FOGGO.

THE journal, from which the following monthly results are extracted, is kept about 20 feet above the level of the sea, and a few hundred yards distant from it. The Thermometer is registered at 9 A. M. and 9 P. M.; the Barometer at 9 A. M. Noon, 4 P. M. and 9 P. M.; the Rain-Gauge and Wind-Vane at Noon. The Hygrometrical observations are made by means of two Thermometers, one of which has its bulb covered with silk, and moistened with water; their indications are registered at noon.

SEPTEMBER 1825.

Results.

1. Temperature.	Fahr. Ther.
Mean of the month,	
Maximum by Register Thermometer,	69.000
Minimum by ditto,	40.000
Range,	29.000
Mean of the extremes,	54.500
2. Pressure.	Inches.
Mean of the month,	
Maximum observed,	30.300
Minimum observed,	29.300
Range,	1.000

3. Humidity.	Fahr. Ther.
Mean difference between the two Thermometers, ...	4°.700
Maximum ditto, ...	13.000
Minimum ditto, ...	0.000

4. Rain, ... 1.32 inchesin 14 days.

5. Winds,...NE. 3, E. 4, SE. 1, SW. 4, W. 8, NW. 1, Var. 9 days.

Remarks.

3*d*.—This day was particularly fine: the brightest sunshine prevailed. The following thermometrical observations were made about 2 P. M.

Temperature of air in the shade, - -	66°.0
of dew-point, - -	47.0
of garden mould exposed all day to the sun's rays at the surface,	121.0
of the same, at the depth of 2½ inches,	81.5
1 foot,	76.0
18 inches,	67.0
2 feet,	63.0
of garden mould always in the shade, at the surface, - -	62.0
of the same, at the depth of 2 inches,	61.0
1 foot,	60.0
18 inches,	59.5
2 feet,	58.0

⁂ In page 67. line 9. of this Number, in the paper on Solar Radiation, the temperature of the air in Mr Campbell's observation should be stated 28°.

4*th*.—This evening, at sunset, there was a gorgeous display of colour in the west. Amongst the numerous tints that appeared, the green was particularly distinct, and remained so for a considerable time. The sky was filled with rather dense *cirro-strati*.

10*th*.—Since the 4th, the weather has been unpleasant; the pressure gradually decreasing, and the temperature of the dew-point rising. To-day, at noon, the latter was 53°; on the 4th it was 41° *. Barometer at 4 P. M. 29.30.

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* During the summer months, our observations on the dew-point were made by means of a contrivance similar in all respects to that which Mr Thomas Jones has proposed in a paper read lately before the Royal Society of London, as a new Hygrometer. We used a common thermometer, with a bulb blown of black glass, the upper half of which was covered with muslin, and surrounded with a rim of silver, fitting closely the largest circumference, and so hollowed out, as to be capable of holding a small quantity of a liquid. Sulphuric ether being dropped upon this surface, the whole bulb was quickly cooled, and the deposition was visible on the lower and exposed surface. This instrument is most easily used. Even in the driest weather in July, when we had the dew-point sometimes 27° and 30° below the temperature of the air, we could obtain a deposition with eight or ten drops of ether in the course of two minutes. [illegible]eral, we employed only five or six drops of ether, and completed [illegible]tion in little more than one minute. We had used this instrumen[illegible] before we heard of Mr Jones's invention; but that gentlema[illegible] to the Royal Society before we had completed our desi[illegible]

11*th.*—Between 4 and 6 P. M. we had a thunder-storm. The *nimbi* came from the SSE. and were of a deep bluish-grey colour: the lightning was pale, but vivid. The discharges were accompanied by very violent gusts of wind, and heavy rain. Barometer 29.44, rising; temp. 57°.5. The rain ceased about 7 o'clock: the night was calm and serene. About 10 P. M. an aurora was observed playing with considerable brilliancy. The storm extended over the greatest part of Scotland, but was felt most severely in Perthshire.

12*th.*—At 9 A. M. temp. 59°.0; dew-point 56°.5. At noon, temp. 64°.0; dew-point 56°.5. Very unpleasant weather; much rain; distant thunder heard in the afternoon.

20*th.*—For several evenings past, there have been distinct convergences of the solar beams at sunset. When this beautiful phenomenon is watched for, we find that it is by no means so uncommon as was formerly supposed.

27*th.*—After a day of the brightest sunshine, the sky was overcast towards the evening by small *cirro-cumuli*, arranged in parallel bars, whose direction was nearly north and south. These caused a general dulness, till the sun got very near the horizon, when, suddenly, the rays shooting through a small opening in the clouds, and illuminating their lower surfaces, produced over the whole western sky, quite up to the zenith, the richest golden and crimson tints it is possible to imagine: these, varying in intensity and depth every second, gradually faded, as the sun sunk below the horizon, but had not wholly vanished 15 minutes after he had set. It is worthy of remark, that, whenever the sun's disk disappeared, the mountains, and indeed the whole surface of the earth, assumed a deep purple, approaching to violet colour, which remained till the moon's rays had usurped the dominion of the night. This splendid sunset was observed throughout all Scotland: indeed, it is probable that it was seen in most parts of the island, as we have learned from different accounts, that it bore the same characters in Caithness that it did in Cumberland.

30*th.*—During the last four days, the weather has been very fine. Winds E. and NE. Bar. 29.90 to 30.36.

OCTOBER.

Results.

1. Temperature.	Fahr. Ther.
Mean of the Month,	51°.241
Maximum by Register Thermometer,	68.000
Minimum by ditto,	33.500
Range,	34.500
Mean of the extremes,	50.750
2. Pressure.	Inches.
Mean of the Month,	29.738
Maximum observed,	30.250
Minimum,	29.000
Range,	1.250
3. Humidity.	Fahr. Ther.
Mean difference between the two Thermometers,	3°.500
Maximum ditto,	8.500
Minimum ditto,	0.000

4. Rain, ... 2.6 inches in 20 days.

5. Winds, ...N. 2, E. 1, S. 4, SW. 7, W. 11, NW. 6 days.

Remarks.

This month has been characterised by the prevalence of strong westerly gales, accompanied during the first two weeks by heavy rains, and, towards the latter end, by frosts.

7th.—Much rain fell to-day. Mean pressure 29,605. Mean temp. 54°.5. Dew-point 54°.0. An aurora was seen in the evening: observed synchronously in the north of Scotland.

17th.—Solar Radiation at 9 A. M. 95°. Mean temperature 49°.5. Dew-point 36°.5. Weather variable, showery.

18th.—Temperature of the preceding night 37°.

21st.—The hygrometrical observations of the last four days have illustrated very well the beautiful law, first developed by Mr Anderson of Perth, of the coincidence between the dew-point and the minimum temperature of the night. The following is an abstract of these *:

1825.	TEMPERATURE.					Weather.
	Min.	Max.	Ther.	Dew-p.	Diff.	
October 17.	46°.0	55°.0	49°.0	36°.5	12°.5	Variable; showery.
18.	37.0	55.0	47.0	44.0	3.0	Dull; fine.
19.	43.4	49.0	44.0	37.0	7.0	Clear: then rain.
20.	36.0	42.5	40.0	35.0	5.0	Clear; very fine.
21.	35.0					Ditto, ditto.

29th.—A lunar halo, with a diameter of 90°, was seen to-night formed in *cumulated cirro-strati.* Pressure diminishing.

30th.—Boisterous gale from NW. Maximum temperature 60°.

NOVEMBER.

Results.

1. Temperature.	Fahr. Ther.
Mean of the Month, ...	
Maximum by Register Thermometer, ...	56.500
Minimum by ditto, ...	25.000
Range, ...	31.500
Mean of the extremes, ...	40.500
2. Pressure.	Inches.
Mean of the month, ...	
Maximum observed, ...	30.120
Minimum observed, ...	28.670
Range, ...	1.450

---

* Since we commenced our observations with Mr Jones's hygrometer, it has often occurred to us, that horticulturists might use such an instrument with grea [illegible] in this variable climate. It is now well established, that the temp [illegible] w-point, as observed in the afternoon in any season, is very n [illegible] h the minimum temperature of the succeeding night; ar [illegible] use of Mr Jones's instrument, a frost might be announce [illegible] o admit of the necessary precautions being taken to secure [illegible] plants, &c. We are well assured that no gardener wou [illegible] y in using the instrument.

Art. XXVII.—*Celestial Phenomena from Jan.* 1. *to April* 1826, *calculated for the Meridian of Edinburgh, Mean Ti* By Mr George Innes, Aberdeen.

The times are inserted according to the Civil reckoning, the day beg ning at midnight.—The Conjunctions of the Moon with the Stars given in *Right Ascension.*

JANUARY.

D.	H.	′	″		D.	H.	′	″	
1.	12	12	22	☾ Last Quarter.	20.	8	16	27	☌ ☽ ♄
2.	8	27	6	☌ ☽ *ι* ♍	20.	12	10	5	☉ enters ♒
2.	8	49	30	☌ ☽ ♂	20.	21	16	50	☌ ☽ ζ ♉
3.	4	37	9	Im. I. sat. ♃	20.	21	20	30	Im. I. sat. ♃
4.	23	5	26	Im. 1. sat. ♃	21.	13	25	51	☌ ☽ η ♊
4.	23	34	9	☌ ☽ δ ♏	21.	16	54	38	☌ ☽ μ ♊
7.	4	9	30	☌ ☽ 1 μ ♐	21.	19	35	47	☌ ☽ ν ♊
7.	4	45	7	☌ ☽ 2 μ ♐	22.	1	55	19	Im. IV. sat. ♃
7.	5	15	0	☌ ☽ ♀	22.	6	18	13	Em. IV. sat.
7.	7	46	0	☌ ☽ ☿	22.	10	46	22	☌ ☽ ζ ♊
8.	1	47	56	Im. II. sat. ♃	22.				☿ greatest elo
8.	2	20	0	☌ ☿ ♀	22.	19	15	0	☌ ☿ φ ♐
8.	9	32	36	● New Moon.	24.	0	1	5	○ Full Moon.
8.	13	29	30	☌ ☽ ♅	24.	11	54	47	☌ ☽ 1 α ♋
9.	10	4	45	☌ ☽ β ♑	24.	13	2	30	☌ ☽ 2 α ♋
10.	6	30	27	Im. I. sat. ♃	25.	8	11	5	☌ ☽ ο ♌
10.	16	47	30	☌ ☉ ♅	25.	16	48	31	☌ ☽ π ♌
12.	0	58	46	Im. I. sat. ♃	26.	2	46	0	☌ ☽ 1 ν ♐
15.	4	22	33	Im. II. sat. ♃	26.	4	45	32	Im. I. sat. ♃
16.	4	35	3	☽ First Quarter.	26.	19	47	30	☌ ☽ ♃
18.	2	49	43	☌ ☽ δ ♈	26.	23	11	6	Im. III. sat.
19.	2	52	8	Im. I. sat. ♃	27.	2	33	38	Em. III. sat.
19.	3	41	19	☌ ☽ Δ ♉	27.	23	13	56	Im. I. sat. ♃
19.	13	9	0	☌ ☽ 2 κ ♉	29.	13	40	0	☌ ☽ ι ♍
19.	22	36	20	Em. III. sat. ♃	30.	12	29	0	☌ ☽ ♂
20.	6	3	28	☌ ☽ ε ♉	30.	20	9	42	☾ Last Quarter

FEBRUARY.

D.	H.	′	″		D.	H.	′	″	
1.	2	32	54	☌ ☽ 2 λ ♎	4.	14	17	55	☌ ☽ d ♐
1.	5	28	38	☌ ☽ δ ♏	5.	0	21	30	☌ ☽ ♅
1.	22	50	27	Im. II. sat. ♃	5.	5	7	40	☌ ☽ ☿
3.	3	9	36	Im. III. sat. ♃	5.	18	26	26	☌ ☽ β ♑
3.	5	0	[illegible]	[illegible]ry near ♅	6.	9	2	0	☌ ☽ ♀
3.	11	25	[illegible]	[illegible] ♐	7.	0	24	16	● New Moon.
3.	12	2	[illegible]	[illegible] ♐	7.	19	53	28	Im. IV. sat. ♃
4.	1	7	[illegible]	[illegible] ♃	8.	0	12	49	Em. IV. sat. ♃

Times of the Planets passing the Meridian.

JANUARY.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	12 8	10 53	6 44	4 24	22 19	12 46
5	11 21	10 59	6 34	4 8	22 6	12 30
10	10 50	11 6	6 25	3 48	21 41	12 10
15	10 33	11 14	6 16	3 28	21 20	11 50
20	10 27	11 21	6 2	3 6	20 59	11 32
25	10 29	11 28	5 53	2 45	20 38	11 13

FEBRUARY.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	10 38	11 37	5 38	2 15	20 10	10 48
5	10 45	11 43	5 29	1 58	19 54	10 33
10	10 56	11 49	5 17	1 37	19 35	10 15
15	11 8	11 54	5 5	1 16	19 16	9 56
20	11 21	11 59	4 52	0 53	18 55	9 37
25	11 34	12 2	4 38	0 31	18 35	9 18

MARCH.

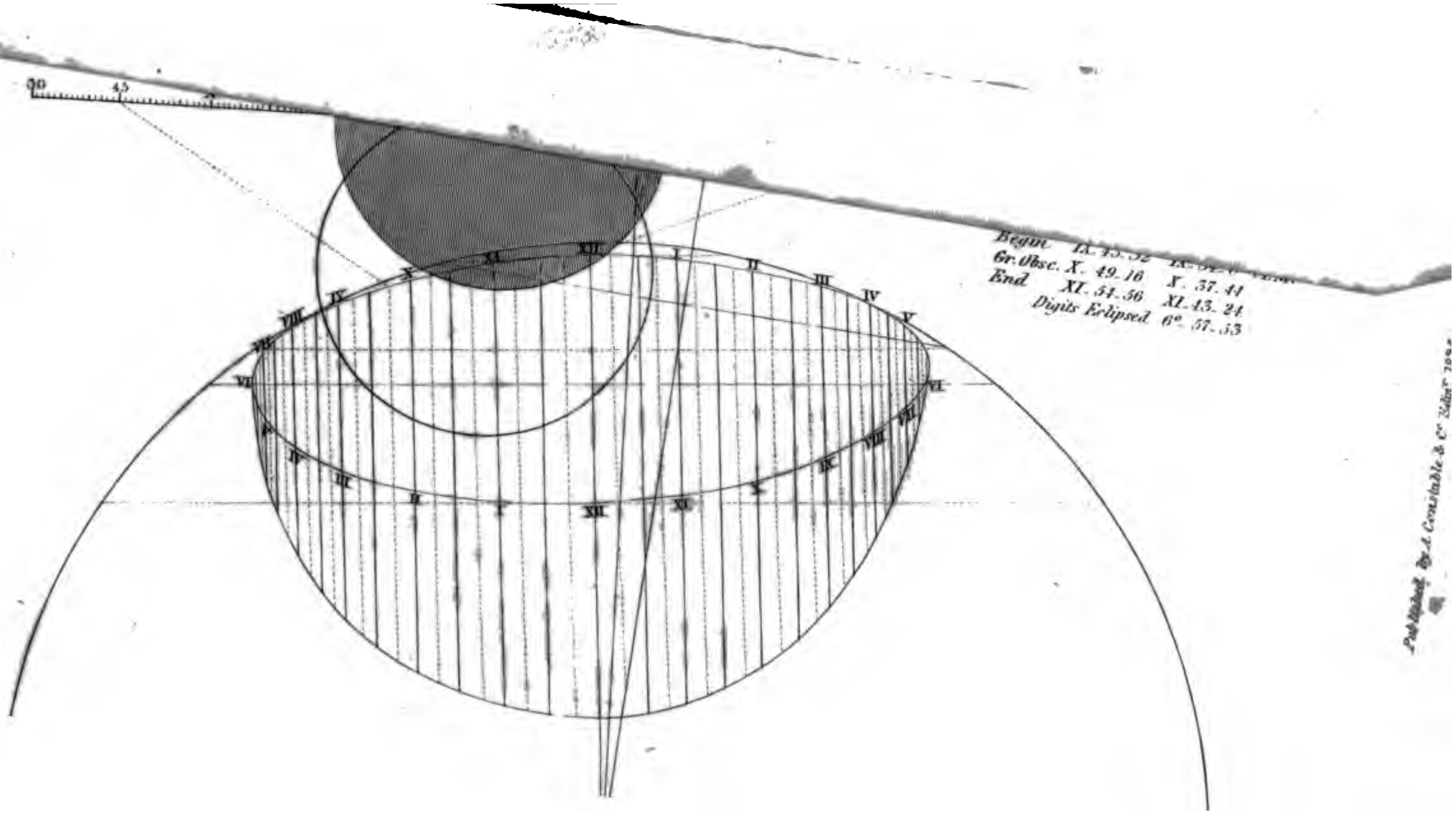
	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	11 46	12 7	4 28	0 9	18 21	9 5
5	11 58	12 10	4 16	23 51	18 9	8 49
10	12 8	12 13	4 1	23 29	17 48	8 26
15	12 28	12 15	3 44	23 7	17 27	8 8
20	12 44	12 18	3 28	22 44	17 9	7 49
25	12 58	12 22	3 10	22 24	17 51	7 30

SOLAR ECLIPSE OF 1826. (Pl. VII.)

On the 29th of November, there will be an eclipse of the Sun, which will be visible. The following are the elements, as obtained by using the Solar Tables of *M. Delambre*, and the Lunar Tables of *M. Damoiseau.*

	D.	H.	′	″
True time of Eclip. Conjunct. at Edin. M. Time,	Nov. 29.	11	12	41,99
Equation of *Mean* to *Apparent time*, at conjunction, -			11	31,76
True time of Ecliptic conjunction, *Apparent time*, -		11	24	13,75
Longitude of the Sun and Moon, from true Equinox, -		246°	46	19,84
Obliquity of the Ecliptic, - - - -		23	27	36,86
Sun's Declination south, - - - -		21	27	34,17
——— Right Ascension, - - - -		244	55	38,92
——— horary motion in Longitude, - - -			2	32,19
——— ——— ——— in Right Ascension, - -			2	41,05
——— semidiameter, - - - -			16	15,15
——— horizontal parallax, - - - -				8,93
Horary decrease of the Equation of time, - -				0,875
Moon's Latitude North, increasing, - - -		1	12	29,55
——— Equatorial horizontal parallax, - -		1	1	23,84
——— H[illegible]emidiameter, - - -			16	43,85
——— ho[illegible]Longitude at the instant of conjunction,			38	5,447
——— —[illegible] ——— for the hour which precedes,			38	5,511
——— —[illegible] ——— for the hour which follows,			38	5,383
——— ho[illegible]tude at the instant of conjunction,		+	3	25,904
——— —[illegible] ——— for the hour which precedes,		+	3	26,181
——— —[illegible] ——— for the hour which follows,		+	3	25,627
Angle of th[illegible]th the Ecliptic, - -		5	30	36,4
Horary m[illegible]om the Sun in the Relative Orbit,			35	43,62

PLATE VII.



Begun IX. 45. 52
Gr. Obsc. X. 49. 16 X. 37. 44
End XI. 54. 56 XI. 43. 24
Digits Eclipsed 6°. 57. 53

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Table of General Data.

Apparent Time at Edinburgh.	Sun's true Longitude.	Sun's Right Ascen.	☽'s hourly mot. in Longitude.	☽'s ½ hourly mot. in Longitude.	Moon's true Longitude.	☽'s hourly mot. in Latitude.	☽'s ½ hourly mot. in Latitude.	Moon's true Latitude.
D. H. ′ ″	° ′ ″	° ′ ″	′ ″	′ ″	° ′ ″	′ ″	′ ″	° ′ ″
29. 9 24 15,500	246 41 15,660	244 50 16,820	38 5,9574	19 2,9787	245 30 8,4676	3 28,1200	1 44,0600	1 5 35,6640
9 54 15,062	246 42 31,705	244 51 37,345	38 5,7341	19 2,8670	245 49 11,4463	3 27,1505	1 43,5752	1 7 19,7246
10 24 14,625	246 43 47,750	244 52 57,870	38 5,5746	19 2,7873	246 8 14,3133	3 26,4580	1 43,2290	1 9 3,2098
10 54 14,187	246 45 3,795	244 54 18,395	38 5,4789	19 2,7394	246 27 17,1006	3 26,0425	1 43,0212	1 10 46,5288
Conj. 11 24 13,75	246 46 19,84	244 55 38,92	38 5,447	19 2,723	246 46 19,84	3 25,904	1 42,952	1 12 29,55
11 54 13,312	246 47 35,885	244 56 59,445	38 5,4151	19 2,7075	247 5 22,5475	3 25,7655	1 42,8827	1 14 12,4328
12 24 11,875	246 48 51,930	244 58 19,970	38 5,3194	19 2,6597	247 24 25,2072	3 25,3500	1 42,6750	1 15 55,1078
12 54 11,437	246 50 7,975	244 59 40,495	38 5,1599	19 2,5799	247 43 27,7871	3 24,6575	1 42,3287	1 17 37,4365
13 24 11,000	246 51 24,020	245 1 1,020	38 4.9366	19 2,4683	248 2 30,2554	3 23,6880	1 41,8440	1 19 19,2805

In obtaining the following results, Taylor's Tables to every second of the quadrant, and Hutton's Tables of the Logarithms of Numbers, have been used. It may be proper to state, that the Sun's semidiameter, as found from the Tables, has been diminished 3″.5 for Irradiation; and that of the Moon diminished also 2″ for Inflexion, after the augmentation corresponding to the altitudes at the respective instants has been applied. These affect the time of the commencement 12″,6; that of the end 13″,3; and the digits eclipsed at greatest obscuration 29″,5.

	For the Beginning.		For the Apparent Conjunction.		For the End.	
	H. ′ ″	H. ′ ″	H. ′ ″	H. ′ ″	H. ′ ″	H. ′ ″
Instants assumed, *Apparent time*,	9 45 15,194	9 46 15,179	10 49 14,260	10 50 14,246	11 54 13,312	11 55 13,297
Sun's Longitude, ...	246° 42 8,891	246 42 11,426	246° 44 51,121	246 44 53,656	246° 47 35,885	246° 47 38,420
—— Right Ascension, ...	244 51 13,187	244 51 15,872	244 54 4,974	244 54 7,658	244 56 59,445	244 57 2,129
℞ of the Meridian, ...	211 10 1,097	211 25 3,557	227 12 38,874	227 27 41,348	243 30 19,125	243 45 21,584
Moon's true Longitude, ...	245 43 28,496	245 44 6,592	246 24 6,602	246 24 44,694	247 5 22,547	247 6 0,638
——— true Latitude, ...	1 6 48,269	1 6 51,725	1 10 29,187	1 10 32,616	1 14 12,433	1 14 15,860
Altitude of Nonagesimal, ...	29 1 19,0	28 55 22,1	22 39 8,6	22 33 10,7	16 28 43,0	16 23 25,0
Longitude of Nonagesimal, ...	172 37 35,3	172 50 1,4	187 21 38,9	187 36 4,8	207 49 49,2	208 16 29,9
Parallax in Longitude, ... +	28 27,43	28 19,62	20 15,60	20 7,63	11 0,97	10 51,46
——— in Latitude, ... —	53 24,02	53 27,06	56 21,09	56 23,52	58 32,65	58 34,23
Apparent diff. Long. ⊙ and ☽,	30 12,96	29 45,22	28,92	1,34	28 47,64	29 13,68
Moon's Apparent Lat. North, ...	13 24,25	13 24,66	14 8,10	14 9,10	15 39,78	15 41,63
Moon's semidiameter, ...	16 44,51		16 45,58		16 46,00	
Appar. mot. ☽ from ⊙ in 60 sec.	25,16		27,58		23,79	
Errors, from Instants assumed,	— 7,18	+ 17,98	— 28,92	— 1,34	— 10,95	+ 12,84
TIMES obtained, ...	9h 45′ 32″,384		10h 50′ 17″,174		11h 54′ 40″,922	

The final results are as follows:

	D.	Apparent Time. H. ′ ″	Mean Time. H. ′ ″
The Eclipse begins,	Nov. 29.	9 45 32,38	9 33 59,91
Greatest Obscuration,	-	10 49 9,95	10 37 37,73
Visible Conjunction,	-	10 50 17,17	10 38 44,97
End of the Eclipse,	-	11 54 40,92	11 43 9,46

Digits eclipsed, 6 dig. 58′ 10″,36, on the north part of the Sun's disc.

The Moon will enter the Sun's disk on the west limb, at 37° 56′ 11″ from his zenith, in reference to the horizon.

Calculation of the Lunar Elements for the 29th November 1826, *at* 11^{h} 34′ 49″51 mean time *at Paris.—From the Lunar Tables of* Damoiseau; *Paris*, 1824.

	Mean Longitude of the Moon. *u*	Mean Anomaly of the Moon. *x*	Mean Long. ☽ — Mean Long. ⊙. *t*	Sun's Mean Anomaly. *z*	Mean Long. ☽ — Long. ☊. *y*	♀ — ♂	♃ — ♂
1826,.........	205° 8748″,3	379° 9348″	294° 6485″	0° 1854″	327° 6673″	156	57
Sec. Equat....	2,23	10,4	2		1		
November,...	50 6938,9	13 0639	117 7642	332 9138	68 5808	208	95
29 days, ...	9 9323,3	6 4664	379 2678	30 6631	11 5798	19	372
11 hours,......	6 7102,0	6 6535	6 2083	0 5019	6 7372		
34 minutes,...	3456,8	3427	3198	259	3471		
49,51 sec.......	83,87	83	77	6	84		
Mean Long. Sum Equat. +	273 5655,40 6225,66	6 4706,4 6225,7	398 2165 6225,7	364 2907	14 9207 6225,7	383	124
	274 1881,06 *v*	7 0932,1 $\bar{x}$	398 8390,7 $\bar{t}$		15 5432,7 $\bar{y}$		

Arguments of the Longitude, Parallax, and horary motion in Long.		Equations of Longitude.	Equations of Parallax.	Equations of hor. mot. in Lo	
				First Order.	Second Or
x	6° 4706″,4	7° 7527″,12	1151″,24	1329″,721	2″,8034
t	398 2165	7177,96	172,72	255,853	1,2310
z	364 2907	3206,16	0,16	0,246	
y	14 9207	697,64	…………	2,633	0,3400
$t+x$	4 6871,4	69,39	…………	2,837	0,0288
$t-x$	391 7458,6	530,30	0,00	0,093	
$2t-x$	389 9623,6	1 1912,53	210,89	231,808	0,5599
$4t-x$	386 3953,6	93,82	3,60	6,104	0,0484
$2t-3x$	377 0210,8	25,51	0,03	0,029	
$2t+x$	2 9036,4	620,39	19,14	32,322	0,2091
$x+z$	370 7613,4	486,85	0,29	0,361	9,0112
$x-z$	42 1799,4	736,30	6,61	7,163	0,0076
$2x+z$	377 2319,8	32,11	…………	0,028	
$2x-z$	48 6505,8	50,59	…………	0,124	
$t+z$	362 5072	25,10	…………	0,952	
$t-z$	33 9258	46,07	…………	0,587	
$2t+z$	360 7237	120,78	0,19	0,262	0,0051
$2t-z$	32 1423	758,30	10,99	16,316	0,0379
$2t+z-x$	354 2530,6	146,92	0,16	0,196	
$2t-z+x$	38 6129,4	71,27	1,10	2,184	0,0063
$2t-z-x$	25 6716,6	890,05	8,23	9,240	0,0119
$2t-z-2x$	19 2010,2	36,08	…………	0,000	
$2t-2z-x$	61 3809,6	42,08	…………	0,237	
$2y+x$	36 3120,4	64,02	…………	0,633	0,0904
$2y-x$	23 3707,6	78,13	0,16	0,074	
$2t+2y-x$	19 8037,6	20,50	…………	0,039	0,0160
$2t-2y+x$	373 0622,4	28,78	…………	0,007	
$t+y$	13 1372	10,74	…………	0,039	0,0169
$t-y$	383 2958	84,36	0,07	0,040	
$u-y$	258 6448,4	4,27			
♀ — ♁	383 ………	4,24			
♃ — ♁	124 ………	0,00			
x and t		21,98	…………	0,357	
x and z		25,15	…………	0,178	
x and y		10,91	…………	0,130	0,0291
x and $t+z$		17,65	…………	0,159	
x and $t-z$		3,40			
x and $2t+z$		16,00	……… ..	0,390	
x and $2t-z$		6,40			
x and $t+y$		3,32	…………	0,026	
x and $t-y$		4,87			
z and $2t-x$		14,21			
z and $4t-x$		16,20	…………	0,475	
z and $t-y$		16,82			
$2t-z$ and t		3,53	…………	0,175	
$t-y$ and t		0,86			
		10 5759,66	1585,58	1902,018	5,4530
Constant quantities,		— 9 9534	+ 9784,3	+ 5151,83	— 5,650
Sum of Equations,		+ 0 6225,66	Eq. par. 1° 1369,88	7053,848	— 0,1970
Nutation,		[illegible]	½ diam. 3098,29	± 0,197	
$u-y$ …… + 44″,39		[illegible]			
t and u …… — 2,32		[illegible]			
Mean Long.		[illegible]		horary motion 7054,045	for the ho preceding
True Longitude,		[illegible]		7053,651	for the ho following.

Arguments of the Latitude and horary motion in Latitude.		Equations of Latitude.	Horary motion in Latitude.	
			First Order.	Second Ord.
$\overline{y}$	15° 5432",7	7° 1041",20	1084",509	1",9820
$2\overline{t}-\overline{y}$	382 1348,7	1177,62	25,905	0,0663
$\overline{x}+\overline{y}$	22 64	2,64	0,003	
$\overline{x}-\overline{y}$	391 55	50,32		
$2\overline{x}-\overline{y}$	398 64	77,63	1,479	
$2\overline{t}-\overline{y}+\overline{x}$	389 23	4,64	0,180	
$2\overline{t}-\overline{y}-\overline{x}$	375 04	67,47	0,115	
$2\overline{t}-\overline{y}-2\overline{x}$	367 95	24,72	0,346	
$\overline{y}+x$	379 83	50,61	1,499	
$\overline{y}-x$	51 25	21,67	0,210	
$2\overline{t}-\overline{y}+x$	346 43	55,47	0,094	
$2\overline{t}-\overline{y}-x$	17 84	86,64	0,976	
$2\overline{t}-\overline{y}-2x$	53 55	3,77		
v	274 19	47,42	0,347	
$\overline{y}$ and $\overline{t}$		9,66	0,231	
x and $\overline{x}-\overline{y}$		3,04		
$\overline{x}$ and $2\overline{t}-\overline{y}-x$		3,12		
		7 2727,64	1115,894	2,0483
Constant quantities,...		—5 9302,2	— 566,310	—2,6850
Latitude, North,...		1 3425,44	+ 549,584	—0,6367

549",584 × 1,15634 = 635",506
—0,6367 × 1,3426 = —0,855

Horary motion for the hour preceding,...... 636,361
———— ———— for the hour following,...... 634,651

Art. XXVIII.—*Proceedings of the Royal Society of Edinburgh.*

Nov. 28.—At a general meeting of the Society the following Office-bearers were elected for the ensuing year:

Sir Walter Scott, Bart. President.

Vice-Presidents.

Right Hon. Lord Chief-Baron.	Lord Glenlee.
Dr T. C. Hope.	Professor Russell.

Dr Brewster, General Secretary.

Thomas Allan, Esq. Treasurer.

James Skene, Esq. Curator of the Museum.

PHYSICAL CLASS.

Alexander Irving, Esq. President.
John Robison, Esq. Secretary.

COUNSELLORS.

Sir William Arbuthnot, Bart.
James Jardine, Esq.
Sir William Forbes, Bart.
Dr Home.
Professor Wallace.
Dr Edward Turner.

LITERARY CLASS.

Henry Mackenzie, Esq. President.
P. F. Tytler, Esq. Secretary.

COUNSELLORS.

Sir William Hamilton, Bart.
Rev. Dr Lee.
Right Hon. Lord Advocate.
Sir Henry Jardine.
Sir John Hay, Bart.
Dr Hibbert.

ART. XXIX.—*Proceedings of the Wernerian Natural History Society.*

THIS Society met for the winter 1825–6 (its eighteenth session), on Saturday, 19th November last.

Mr Henry Witham of Lartington, read a notice of the occurrence of the common Cockle, *Cardium edule*, in a living state, in fresh-water ditches, at Cocklesbery in Yorkshire, at the distance of forty miles from the sea, and greatly above its present level. He exhibited specimens of the shells, from which he had, on the spot, extracted the living animal: these shells did not differ in the slightest degree from those of the cockle which inhabits our sandy sea-shores. The animal, however, Mr Witham mentioned, had somewhat less of the salt taste or fishy flavour than the cockles sold in our markets.

A memoir by Mr David Don, Librarian of the Linnean Society, "*On the Classification of the Genera Gnaphalium and Xeranthemum of Linnæus*," was next laid before the meeting.

There was then read the first part of Mr Thomas Buchanan's sketch of the comparative anatomy of the Organ of Hearing, containing remarks on the structure of the ear in the Shark tribe, illustrated by preserved specimens.

There wa[illegible] read a communication by Mr Blackadder, regarding th[illegible] a hard rock of Conglomerate in the midst of the [illegible]ls near Edinburgh; and Professor

Jameson gave an account of a Table of Colours, arranged for naturalists by the Reverend Lansdown Guilding of St Vincent's, intended as supplementary to Mr Syme's treatise on colours.

3d Dec. 1825.—The Secretary read Dr T. S. Traill's account of the Anatomy of the Trumpeter-bird, *Psophia crepitans.*

Dr R. E. Grant then communicated some notices of the habits of *Tritonia arborescens,* particularly the power possessed by that molluscous animal of producing a peculiar and very audible sound; and the Doctor at the same time exhibited specimens, which had been kept alive and active for more than three weeks, in a jar filled with sea-water, the water having been occasionally renewed.

Professor Jameson communicated some remarks on the existence of many mineral substances, in very minute portions, in the ocean and in the atmosphere.

At the same meeting, the following gentlemen were elected office-bearers of the Society for the following year:

ROBERT JAMESON, Esq. *President.*

VICE-PRESIDENTS:

Robert Bald, Esq.
Sir William Jardine, Bart.
Dr Robert Graham.
Rev. Dr A. Brunton.

Treasurer, A. G. Ellis, Esq.
Secretary, P. Neill, Esq.
Painter, P. Syme, Esq.
Librarian, James Wilson, Esq.

COUNCIL:

Wm. Drysdale, Esq.
Gilbert Innes, Esq.
Dr Robert Knox.
G. A. W. Arnott, Esq.
Dr Andrew Coventry.
John Stark, Esq.
Dr R. E. Grant.
Dr John Boggie.

ART. XXX.—*Proceedings of the Northern Institution, Inverness.*

Sept 16. 1825.—AT this meeting the following gentlemen were elected

HONORARY MEMBERS.

Sir James Macgrigor, Knight, F. R. S. &c.
Dr Traill of Liverpool.
Dr Thomas Thomson, Professor of Chemistry, University of Glasgow.

Dr Ure of the Andersonian Institute of Glasgow.
Robert Jameson, Esq. Professor of Nat. Hist. University of Edin.
David Brewster, Esq. LL. D. &c.

And several corresponding and ordinary members.

The papers read were,

1. Original letter of Simon, Lord Fraser of Lovat. Communicated by John Anderson, Esq. W. S.

2. Evidence respecting a sudden commotion of Loch Ness about the time of the Lisbon Earthquake in 1755. From Mrs Grant of Duthil.

3. Notice of a subterranean passage lately discovered in Glen Shiel. By Mr Mactavish, solicitor.

4. Remarks by the Secretary on an ancient custom-house seal of the conjoined burghs of Inverness and Cromarty, supposed to be of the age between Alexander II. and Robert III.

5. A paper from Mr Fraser, Croyard, on the sections lately made, by order of Mr Fraser of Lovat, of a vitrified fort on his property, laid on the table, but the reading of it postponed till next meeting.

ART. XXXI.—SCIENTIFIC INTELLIGENCE.

ASTRONOMY.

1. *Comets.*—At a meeting of the Astronomical Society of London, held on the 11th November, the President took the opportunity of calling the attention of the members to the remarkable circumstance of the appearance of no fewer than *four* comets during the recess, an occurrence unparalleled in the history of astronomy. The *first* of these (he observed) was discovered by M. Gambart, at Marseilles, on May 19. in the head of *Cassiopea.* The *second* by M. Valz, at Nismes, on July 13, near χ *Tauri.* The *third* by M. Pons, at Florence, on August 9, in *Auriga.* The *fourth* (which was the most interesting and important of the whole, since it had been the object of solicitude at every observatory, and was anxiously expected and looked after by every astronomer) was discovered about July or August last. The F remarked, that this last comet (which is better known of the *comet of Encke*) has now made thirteen revc he last forty years; six of which

have been regularly observed by astronomers. It was first seen in 1786; afterwards in 1795, 1805, 1819, and 1822, and in the present year. It makes a complete revolution in about 1207 days, or 3½ years.

ACOUSTICS.

2. *A Table shewing the Results of Experiments on the Velocity of Sound, as observed by different Philosophers*.*

Names of Observers.	*Time when made.*	*Country where made.*	*Length of Basis in Feet.*	*Velocity of Sound per Second in Feet.*
Mersenne,[1] - -		France,		1469.88
Florentine Philosophers,[2]	1660	Italy,	5905.8	1184.44
Walker,[3] - -	1698	England,	2624.8	1305.83
Cassini, Huygens, &c.[4]		France,	6906.50	1151.63
Flamstead and Halley,[5]		England,	16405.0	1141.78
Derham,[6] - -	1704 1705	England,	5249.6 to 6562.	1141.78
French Academicians,[7]	1738	France,	75177.55 & 93593.8	1092.57 at 32° F.
Blanconi,[8] - -	1740	Italy,	7874.0	1043.35
La Condamine,[9] -	1740	Quito,	67401.58	1112.25
La Condamine,[10] -	1744	Cayenne,	129366.54	1174.59
T. F. Mayer,[11] -	1778	Germany,	3702.40	1105.69
G. E. Muller,[12] -	1791	Germany,	8530.6	1108.97
Epinoza and Banza,[13]	1794	Chili,	53627.94	1168.50
Benzenberg,[14] -	1809	Germany,	29765.23	1092.57 at 32°
Arago, Mathien Prony,[15]	1822	France,	61065.97	1086.0
Moll, Van Beek, and Kuytenbrower,[16]	1823	Netherlands,	5797290.76	1089.7445 at 32° F. dry air.

[1] Mersenne de Arte Ballistica, prop. 39.

[2] Tentamina Experim. Acad. del. Cimento, L. B. 1738, part ii. p. 116.

[3] Phil. Trans. 1698, No. 247.

[4] Duhamel, Hist. Acad. Reg. l. ii. sect. 3. cap. ii.

[5] Phil. Trans. 1708 and 1709.

[6] Id. ibid.

[7] Mem. de l'Academie des Sciences, 1738 and 1739.

[8] Comment. Bononienses, vol. ii. p. 365.

[9] La Condamine, Introduction Historique, &c. 1751, p. 98.

[10] Mem. de l'Acad. Royale des Sciences, 1745, p. 488.

[11] J. T. Mayer, Praktische Géométrie, Göttingen, 1792, b. i. p. 166.

[12] Muller, Götting. Gelehrt. Anzeige, 1791, st. 159, et Voigts Magazin, &c. b. 8. st. i. p. 170.

[13] Annales de Chimie et de Phys. t. vii. p. 93.

[14] Gilbert's Annalen, neue Folge, b. v. p. 383.

[15] Connoissance des Tems, 1825, p. 361.

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* From Von Moll's Memoir on the Velocity of Sound in *Phil. Trans.* for 1824, part ii.

GEOGRAPHY.

3. *Expedition to Explore the Shores of the Frozen Sea, and the North-East Coast of the Continent of Siberia.*—Baron Wrangel, and Lieutenant Arjon, who were sent in 1821 upon an expedition to Siberia, the object of which was to determine geographically the shores of the Frozen Sea, and the north-east of the vast continent of Siberia, as far as the country of the Tschutsches, returned to Petersburg some weeks ago. M. Kyber, who accompanied the expedition as physician and naturalist, has arrived at Moscow, where he has been detained by sickness. The publication of the results of this important expedition is looked for with the greatest anxiety.—*Leips. Lit. Zeit. No.* 93. 1825.

4. *Captain Parry's last Voyage.*—Our readers may probably expect from us some details in regard to Captain Parry's last voyage; but as the journals are still in the possession of the Admiralty, we have it not in our power to gratify them by any official and consequently accurate information. The various accounts published in the daily journals we know are incorrect; and, therefore, cannot be recorded in this work.

5. *East Coast of West Greenland, formerly inhabited by Europeans.*—Early history informs us that a part of the east coast of West Greenland was colonized by Norwegians from Iceland. The colony appears to have been considerable, and to have extended northward to Lat. 65° or 66°. Some authors, and particularly a writer in the Edinburgh Review, maintains that no such colony ever existed; on the contrary, that the Norwegians landed and colonized the West, not the East, coast of Old Greenland. The late observations of Scoresby, and the details given by Giesecké, in a memoir published in the memoirs of the Royal Irish Academy, demonstrate the futility of the opinion just mentioned. Giesecké, who spent eight years in Greenland, tells us, he met with upwards of fifty Norwegian houses, in the fiords or firths of South and East Greenland, fragments of church-bells, and skulls of the Caucasi [illegible] an race of man. In the language of the Greenl [illegible] ted many Scandinavian or Icelandic words, used [illegible] a proof that there existed a friendly

intercourse between both nations. Several plants foreign to this part of the Arctic Flora were met with, probably imported by the Norwegian settlers, such as the *Sorbus aucuparia.* In reference to the destruction of the colonists, our author remarks; "All the ruins of Norwegian houses were surrounded by immense masses of rocks, probably precipitated from the summits of the adjacent mountains, and heaped together in the most fantastic groups. Places of desolation of this kind are frequently met with among the mountains, connected with the sea by waterfalls, which are precipitated with tremendous velocity from the rocks, covered with glaciers. I have no doubt that such changes, caused by the bursting of glaciers, and the subsequent inundations, have produced these scenes of desolation; and that perhaps the Norwegian settlers perished, and were buried in the ruins occasioned by such destroying powers."

6. *Edinburgh Geographical and Historical Atlas.*—It is intended in this work to exhibit, by means of numerous maps, and four octavo volumes of letter-press, a view of the present state of our knowledge, in regard to the physical, political, and statistical geography of this globe. To ensure its wide circulation the publisher announces that it is to appear in monthly parts, and to be sold at a comparatively low rate.

CHEMISTRY.

7. *Evolution of Light during Crystallisation.*—It is known, through the experiments of M. Buchner of Mayence, that benzoic acid and acetate of potash emit light during their crystallisation. Berzelius, in his lately published Annual Report of the Progress of Science, tells us, that Herman observed sulphat of cobalt to give out light during crystallisation, and that a similar phenomenon was observed during the crystallisation of fluat of soda. Wöhler mentions a striking display of this property he noticed in the laboratory of Berzelius, where, during the crystallisation of sulphat of soda, light was given out for two hours. Even masses of the salt taken in the hand continued to shine in the dark, and when pieces were rubbed together the light became stronger. When the solution was stirred with a glass-rod, or a glass-rod was drawn across the crust of crystals under the solution, the whole streak was luminous.

8. *Light emitted during the Friction of Crystals.*—It is well known that many crystallised substances, when rubbed together, or broken across, give out a light more or less intense. It is said by Olof Wasserström, in the Transactions of the Swedish Academy for 1798, that the phosphorescence of the sea, in northern countries, may sometimes be owing to the small and very thin needles of ice, which almost cover the surface of the sea, being broken in pieces by the agitation of the waves, and thus emitting a light, which may assist in giving the luminous character to the water. He also affirms, that masses of sea-ice, when violently struck, give out light. The following passage from Becquerel, on the development of electricity by pressure, in the *Ann. de Chim.* 22. p. 5., is of the same general nature:—"Considering the increased development of electricity in bodies, by the augmentation of pressure, ought we not to refer to this cause certain luminous phenomena, of which the origin is as yet unknown? For instance, it is said, that, in the Polar seas, it frequently happens that the blocks of ice which strike together evolve light. These enormous blocks arriving one against the other, with considerable motion, will be submitted to great pressure, and thus the two blocks be placed in two different electric states. At the moment the compression ceases, the two fluids will recombine, in consequence of the conducting power of the ice; and may not the light disengaged be the result of the combination of the electric fluids? Iron, submitted to successive blows, also becomes luminous: Are not the same phenomena of pressure produced here, as when two masses of ice strike together?

9. *Benzoic Acid in Grasses.*—It is known that Scheele detected benzoic acid in the urine of newly born children; and that, more lately, chemists have found the same acid in the urine of some graminivorous animals, as the cow, the horse, and in that of the rhinoceros. These facts naturally lead us to inquire the source of this acid in the animal kingdom. Some conjecture that it is formed by the organic powers of the animals; while others maintain that it has been derived from without. Th [illegible] on has been in part confirmed by some late expe [illegible] l. He found this acid in an uncom-

bined state in those grasses which have the delightful smell of fresh hay, as the *Anthoxanthum odoratum* and *Holcus odoratus*, two species favourite articles of food with the horse and cow. The benzoic acid in the urine of the newly born child, may possibly be derived from the milk of the mother. If the grasses above mentioned should be found to afford so much acid as to allow of its being economically extracted, they may furnish the arts with an expensive article heretofore imported from abroad.

10. *Formation of Metallic Copper by Water and Fire.*—In making cement-copper in Germany, plates of *solid* copper are obtained, and also reguline copper in the fibrous, capillary, dentiform, reniform, and botryoid external shapes; and in the smelting of some sulphurets of copper, fibrous, lamellar, and crystallised pure copper arc formed.

11. *Effect of Position on Crystallisation.*—Machman, Professor of Chemistry at Christiania, in Norway, in a memoir "on the Effect of the Earth's Magnetism on the separation of Silver," states, that, in the year 1817, when exhibiting, in a syphon-shaped glass-tube, the formation of an *arbor Dianæ*, the tube having accidentally been placed in the direction of the magnetic meridian, he remarked that finer and longer crystals were formed towards the north than towards the south, and yet every thing was the same in both legs of the tube. The solution of nitrate of silver in both legs of the tube, was in communication, while the mercury covered only the bottom of the tube. The experiment was again repeated, in presence of Hansteen, with two syphon-tubes, one parallel, and the other at right angles to the magnetic meridian. The silver began to separate in the tube which was placed in the north and south direction, and shot out into larger, more numerous, and more brilliant radiations in the leg towards the north, than in that towards the south. In the syphon in the east and west direction no change was observed until the expiry of twelve hours. Hansteen afterwards repeated the experiment several times, and always with the same result, and deduced from his experiments the following inferences. 1. The arbor Dianæ is more strikingly developed when the tube is placed in the magnetic meridian, than when in

the east and west direction. 2. When it remains in the magnetic meridian, the silver-tree rises higher in the northern than in the southern leg. 3. The crystals are more acicular, and have a higher metallic lustre, in the northern than in the southern leg of the syphon. The same experiment has been successfully repeated by Dobereiner and Schweigger, from whose Journal the above details are extracted.

12. *Sulphur in Vegetables.*—Sulphur, in combination with different bases, occurs in wheat, barley, rye, oats, peas, beans, maize, millet, rice, and salop. Gum-arabic also contains traces of ammonia and sulphur.

13. *On supposed Hydrates of Sulphur.*—It would appear, from some experiments of Professor Bischof of Bonn, in opposition to the statements of other chemists, that sulphur does not occur in the state of hydrate, when poured in a melted state into water, when precipitated from sulphuret of soda, or in crystals of sulphur. Here Bischof makes a distinction between water of crystallisation and water in true hydrates; the former parts readily from the body containing it under the common pressure of the atmosphere, and therefore more readily under the air-pump; whereas the water of true hydrates does not escape under the air-pump, and often requires the assistance of considerable heat to separate it.

14. *View of the Atomic System, for the Use of Students*; by E. Turner, M. D.—This interesting little work contains a popular and luminous view of the Atomic System, and cannot fail to prove acceptable, not only to the student, but also to the general reader.

15. *Lithia in Spring Water.*—Berzelius has detected, in the Franzbad and Marienbad waters of Bohemia, and in the hot springs of Carlsbad, carbonate of lithia. It is probable that the same substance will be found in the waters of the ocean. The ocean, and the atmosphere, it may be conjectured, will be found to contain minute portions of all the principal materials that ent[illegible] the composition of the solid mass of the globe, an inf[illegible]l on obvious geological and meteorological data.

METEOROLOGY.

16. *Meteoric Stone.*—A meteoric stone, weighing 16 pounds 7 ounces, fell from the air at Nanjemoy, Maryland, 10th February 1825. It was taken from the ground about half an hour after its fall, was sensibly warm, and had a sulphureous smell. It had a hard vitreous surface; its interior was earthy, and of a light slate-colour; and contained numerous hard, brown globules of various sizes, together with small portions of iron-pyrites.

17. *Falling Stars.*—Dr Brandes of Breslau, and several other meteorologists, have for some time past been actively employed in making corresponding observations on falling stars. Although these remarkable meteors, apparently situate beyond the atmosphere of the Earth, at first sight appear to move in every possible direction, yet, according to the observations of Dr Brandes and his friends, it would seem that the most frequent direction is the opposite of that of the Earth in its orbit.

HYDROGRAPHY.

18. *Remarkable Appearance in a Lake.*—On the 19th July 1824, after a storm, the waters of a lake in the district of Lucca became as if soap had been dissolved in them, or lime slacked in them. They continued in this state the whole of the 20th of July; but, on the 21st, an incredible number of fishes, of various sizes, appeared on the surface, which were buried, in order to prevent the occurrence of any contagious disease.—*Ann. de Chim. et Phys.* xxvii. p. 386.

MINERALOGY.

19. *Discovery of Iodine in combination with Silver.*—Iodine was first discovered in marine plants, afterwards in mineral waters, and even in the waters of the ocean. It occurs also, in the various marine molluscous animals, as the Doris, Venus, Ostrea, &c., and even in Sponges and Gorgonia. Very lately, this curious substance has been detected by Vauquelin in combination with silver, in some specimens brought from America.

20. *Platina found in Russia.*—This mineral has been discovered in the Uralian Mountains, and, like the platina of Choco in South America, associated with fragments of greenstone.

The grains are rich in osmium and iridium. At Choco the grains contain osmium, iridium, and palladium; in the Brazils, alone, grains of palladium are found mixed with grains of platina, gold, and diamonds.

21. *Graphite.*—From some late experiments, it remains doubtful whether natural graphite be a pure carbon-metal, or really a combination of carbon and iron.—Vide ***Karsten in Phil. Magazin***, vol. lxvi. p. 290.

22. *Discovery of two new Minerals.*—In the number for November of the Annals of Philosophy, there are descriptions of two new minerals by Mr Levy, to which he proposes giving the names of *Herschelite* and *Phillipsite*, the former in honour of the Secretary of the Royal Society, the latter of Mr W. Phillips, whose contributions to mineralogy are so extensive and valuable.—*Herschelite* occurs in white, translucent, and opaque crystals, sometimes isolated, but generally very closely aggregated, in a manner analogous to that in which the crystals of prehnite are so generally met with. The matrix consisted entirely of small grains and crystals of olivine. A small quantity of the mineral was examined by Dr Wollaston, and found to contain silex, alumina, and potash. These being also the constituents of felspar and amphigene, it might be hence inferred, that the Herschelite is only a variety of one of these minerals, but its crystallographic and other characters shew it to be different from both. The form of the crystals indicates, that they are derived either from a rhomboid or a six-sided prism. No cleavage could be obtained. The specific gravity is 2.11. The fracture is conchoidal, and the substance is easily scratched by the knife. It was brought by Mr Herschel from Aci Reale in Sicily.—*Phillipsite.* This substance accompanies the former, and occurs in minute, white, translucent, and opaque crystals. In specimens from Aci Reale, these crystals are elongated, adhere closely together, radiating from a common centre, and forming globular concretions; in specimens from Vesuvius, they are separated, and accompanied with Comptonite and other minerals. The form of these c[illegible] the same as that of harmotome, and the incidence of [illegible] nearly the same. The hardness, however, is m[illegible]vage is not in the direction of the dia-

gonal planes, as in Harmotome, and the chemical composition differs, Dr Wollaston having found it to consist of silex, alumina, potash, and lime, without the slightest trace of barytes. The primitive form is a right rectangular prism, or a right rhombic prism.

23. *Remarkable Crystals of Pleonaste.*—Dr Fowler has discovered in Orange County, New York, crystals of *Pleonaste,* remarkable on account of their size, their bases measuring from 4 to 16 inches in circumference; they are of a blackish colour, and in this locality, the Doctor adds, they are never less in size than a bullet. In the same situation, crystals of *Serpentine,* in form of a rhomboidal prism, were met with; also large prismatic crystals of *Chromate of Iron,* some of them being one inch broad, and two inches long; green, red, and brown crystals of *Spinel,* in size from a line in diameter to three quarters of an inch on each side of the bases. All these interesting minerals occur imbedded in primitive limestone. In the same district, crystals of *Scapolite* of extraordinary size are met with; Dr Fowler mentions crystals upwards of 24 inches in circumference.

GEOLOGY.

24. *Notice regarding a Phenomenon observed in the Island of Meleda, in the province of Ragusa.*—The Island of Meleda, where the occurrence that we are about to relate took place, is situated in the Adriatic Sea, opposite the territory of Ragusa, of which it forms a part. Its length is seven leagues, and its greatest breadth one. About the middle of the island is situated the valley of Babinopoglie, half a league in breadth, and surrounded with pretty high mountains. A village of the same name occupies the centre of the valley. On the 20th March, at day break, a noise was heard for the first time at Babinopoglie, similar to the report of a cannon; which, although it appeared to be the result of distant explosions, caused a sort of shaking in the doors and windows of the houses of the village. This noise was heard daily after. During the three first months, the inhabitants were undecided regarding the place from whence these noises proceeded; some thinking that a vessel was exercising in the open sea, or in one of the ports of Dalmatia; others that the Turkish Artillery were training in one of the towns of the Ot-

toman frontier. These conjectures serve to shew, that the reports were not accompanied with any local symptoms of earthquake, or any motion of the atmosphere. The Governor of the island posted people on the heights around Babinopoglie to discover, if possible, the direction from which the sound came; but they were unable to observe any constant direction, as the sounds were heard sometimes on one side, sometimes on another, and sometimes over head. The Governor himself went down into some deep and spacious caverns, that existed in the island, but here there reigned a perfect silence. The effect was most sensible at Barbinopoglie, and diminished from this point, so as to be scarcely perceptible at the extremities of the island. There were four, ten, or even a hundred detonations in the day; their loudness increased to such a degree, that they might be likened to the reports of a gun of large caliber. They took place in all seasons, at every hour of the day, whether the weather was fine or stormy, whether the tide was flowing or ebbing, and whether the sea was calm or agitated. It was in the month of August 1823 that they became most violent. No rain had fallen for four months; the brooks were dried up, and the rivers of the mainland were very low. Things went on thus until the month of February 1824. A silence of seven months then ensued; but the reports commenced again in September, and continued until the middle of March 1825, although they were much weaker, and at greater intervals. They then ceased, but it cannot be known whether this silence is to be permanent. There have been intermissions of several months during the phenomenon, but the cessation of the noise was preceded by very loud reports, and before this last cessation they became weaker and weaker. The reports were never accompanied with any luminous appearance; no local modification of the atmosphere was observed during their continuance; the barometer and electrometer manifested no extraordinary movement. Nor was there any true earthquake, although the doors and windows were shaken. The nature of the sound indicated nothing subterraneous, but rather an explosion in the surrounding atmosphere. Dr Stulli of Ragusa, w [redacted] es the above details, supposes these reports to have be by the emission of quantities of gas elaborated b fire, situated beneath the island, or communicating with it, which, on escaping, struck the air with violence, and so produced the reports.—*Bibliotheque Universelle, August* 1825.

25. *Considerations on Volcanoes, by G. P. Scrope, Esq. Sec. Geol. Soc.*—This is the most complete treatise on volcanoes hitherto published in Britain; and, although we differ from the intelligent author in some of his views, we have much pleasure in recommending his work to the particular attention of the geologist.

26. *Comparative durability of Marble and Granite.*—A fragment of a column in the ruins of Capernaum, mentioned by Professor Hall, is of an extremely beautiful granular marble, which has all the freshness and brilliancy of a specimen recently taken from a natural quarry. It has been full proof against the attacks of the elements during the lapse of perhaps 2000 years. Although limestone is softer than granite, it is frequently less liable to decomposition. This remark accords with the observations of several travellers in Egypt, Greece, and Palestine. The feldspar of the granite is affected by the action of air and moisture sooner than either of its other ingredients. "Of all natural substances used by the ancient artists," says Dr Clarke, "Parian marble, when without veins, and therefore free from extraneous bodies, seems to have best resisted the various attacks made upon Grecian sculpture. It is found unaltered, when granite, and even porphyry, coeval as to their artificial state, have suffered decomposition."

27. *Geognosy of Palestine.*—From the observations of Professor Hall, Dr Clarke, and other naturalists, it appears, that Palestine is principally composed of secondary limestone, intermingled with trap-rocks; and the following, among other facts, are illustrations of the truth of this position. The country between Jerusalem and Jaffa is of compact limestone; the hill on which Nazareth is built is of a grey coloured compact limestone; the Field of Blood, mentioned by St Mathew, is of friable limestone; David's Cave, mentioned in I. Samuel xxiv. appears to be situated in limestone; the Mount of Olives is of limestone, in part granular; limestone occurs in the Valley of Jehosaphat;

the rocks around the Pool of Siloah are of limestone; a beautiful granular, foliated limestone or marble occurs at the *Grave* of Lazarus; on Mount Zion, the rocks are of a conchoidal greyish siliceous limestone; Mount Lebanon appears principally composed of limestone; Mount Carmel is interesting, on account of the large balls of quartz contained in the limestone,—these balls have been described as petrified melons, but are merely of quartz in the state of hornstone, and including layers of calcedony, and crystals of quartz; all the rocks around Jerusalem are of compact limestone, and the numerous tombs in the neighbourhood of that city are hewn in hard, compact limestone; Mount Tabor, Bethel, Capernaum, also afforded specimens of limestone to the American missionary, the Reverend Pliny Fisk, to whom Professor Hall was indebted for the collection from the Holy Land, which he has described in the Number of Silliman's American Journal of Sciences and Arts for June 1825.

BOTANY.

28. *Rhizomorphous plants in Mines.*—It appears from observations lately made in Germany, that rhizomorphous plants grow in the most delicate fissures in coal and rocks of the coal-formation, at a considerable distance from the walls of the subterranean galleries, some hundred feet below the surface, and in places where both water and air can occur only in the minutest quantities. In these fissures the plants lose the roundish form they have when encrusting the walls and pillars of the mine, becoming flat, and like the finest paper. The growing of these plants in situations almost without air, and without water, recalls to our attention the chronicled relations of toads, lizards, and other animals found in solid rocks. More of this on another occasion.

29. *Luminous appearance in Mines.*—In a former Number of this Journal, we gave a short account of luminous plants, particularly of the Rhizomorpha. The following notice on the luminosity of the Rhizomorpha, is recorded by the councillor of mines, Erdmann, in the 1st number of the 14th vol. of Schweigger's Jou [illegible] ppearances mentioned were seen on visiting one o [illegible] near Dresden. "I saw the luminous plants he [illegible] beauty; the impression produced by

this spectacle I shall never forget. It appeared on descending into the mine, as if we were entering an enchanted castle; the abundance of these plants was so great, that the roofs, walls, and pillars, were entirely covered with them, and the beautiful light they cast around almost dazzled the eye. The light they give out is like faint moonshine, so that two persons near to each other could readily distinguish the outline of their bodies. The light appears to be most considerable when the temperature of the mines is comparatively high."

30. *Rare Scottish Plants.*—In a walk through the island of Skye, the west of Ross-shire, and Sutherland, to Caithness, in August last, Dr Graham and Mr John Home ascertained the following new stations for some rare Scotch plants. *Apargia Taraxici*, *Arabis hispida glabrous variety*, *Luzula arcuata*, *Aira lævigata vivipara*, *Cerastium latifolium*, on disjointed quartz rock, near the summit of Fonniven, a mountain about 3000 feet high, top of Loch Inchard in Sutherland; the last also on Ben-Hope, on micaceous rock. *Salix stuartiana*, *Carex capillaris*, *Serratula alpina*, *Arabis hispida hairy variety*, on micaceous rocks of Ben-Hope. The *Arabis hispida* is abundant on Fonniven as well as Ben-na-Callich, in Skye; growing, not on damp spots near the sides of rivulets, as has been stated, but always among dry loose stones, at or near the summits. The species is by far most frequently smooth, no hairy specimen but one, picked on Ben-Hope, having been seen. It is said to be frequently hairy in Mull. *Carex limosa*, Batcall Moss, between Loch Inchard and Old Shore. *Carex pulla*, shore south of eastern extremity of Crinan Canal, and Coruisk, top of Loch Scavaig, Skye. *Malaxis paludosa*, side of a stream leading from Ben-na-Callich to Loch Slappen in Skye, about one-fourth of the way up the mountain; in considerable quantity in one small spot. *Stachys ambigua*, abundant near Aird, and at Uig, in Skye. *Betula nana*, low moor between Ben-Hope and Tongue, and at the foot of Ben-Loyal. *Aspidium dilatatum*, a remarkable variety, with long straggling alternate pinnæ, Ben-Loyal, towards Tongue. *Subularia aquatica*, in Sword Loch, near the confines of Sutherland and Ross-shire, and in the river Kerry, at Kerrysdale, Gareloch; in this last situation, it had been previously seen by Dr Woodforde. *Orobanche rubra*, near the Spar

Cave, Loch Slappen, and on the shore at Stenchall, Skye. This plant was, this autumn (1825), for the first time in England, found by Dr Woodforde at the Devil's Frying-pan, Cornwall. *Circæa lutetiana*, Tobermorry, island of Mull. This is the plant of the Flora Britannica, and quite different from the common luxuriant varieties of *Circæa alpina*, whether it be specifically distinct or not. *Primula Scotica*, in great abundance around Westfield, near Thurso. *Scutellaria galericulata* grows in abundance on many parts of the West Coast, on heaps of dry gravel above the high-water mark, and even on a dry stone wall south of the eastern entrance to the Crinan Canal. *Veronica officinalis var. rigida*, cliffs by the shore, near Portree, Skye. Till specimens in flower can be obtained, this may be considered a variety of *V. officinalis*, though there is much reason to believe it distinct. Leaves lanceolate, sharply, rather deeply, and sometimes twice toothed, shining, and very thick and rigid. Stems many, prostrate, rooting, nearly devoid of hairs; common flower-stalks covered with yellow pubescence; spike crowded; capsules more wedge-shaped, and less notched than in *V. officinalis;* slightly hairy. These plants are distinguished from *V. Allionii* by the shape of their leaves, and the depth of the serratures; and they are more rigid than any foreign specimens which Dr Graham has seen.—*R. G.*

31. *Rare Native Plants found in Perthshire.*—Mr David Bishop, a meritorious practical gardener, and keen botanist, has, during the past summer, detected four rare plants in Perthshire. —1. *Pyrola uniflora.*—This was formerly known only as a native of a fir-wood near Brodie in Nairnshire; and having disappeared there, owing to the cutting down of the timber, was regarded as extinct in Britain. At the Perthshire habitat now observed by Mr Bishop, we understand it occurs in considerable abundance.—2. *Lotus minor*. This he has found in two stations; one near Perth, and the other 30 miles to the westward. *L. minor* (like *L. major*), has by some been regarded as only a marked variety of *L. corniculatus ;* but Mr Bishop considers both *major* and *minor* to be specifically different from *corniculatus* [illegible] ks, " *L. corniculatus* is never found in flower after [illegible] f September; while *L. minor* continues in flowe [illegible] of the month, and has at the time

a great number of newly formed flowers, which do not come forward, but fall off. The seeds of *L. corniculatus* are brown and spotted with dark spots; those of *L. minor* brown without spots; those of *L. major* greenish white, and without spots. The long straggling hairs upon the teeth of the *calyx* in *L. major* and *corniculatus* are wanting in *L. minor*. In the barest moors, the stems of *L. corniculatus* creep immediately below the surface for half their length or more, sometimes taking root; these stems can always be traced to the parent root, which descends perpendicularly into the earth: the stems of *L. minor* rest upon the surface of the earth. The strong stems of *L. major* growrather erect, and its root is creeping, white, and tinged with red at the joints. The standard or *vexillum* of the flower in *L. minor* is rounder than in either of the others; that of *L. major* is most elliptical; the *calyx* of *L. minor* is more acuminate towards its base, than in *major* or *corniculatus*. There are marked differences in the taste of these different plants, especially in the roots: the roots of *L. corniculatus* are sweet and pleasant: those of *L. major* feel astringent, like so much oak-bark in the mouth: those of *L. minor*, are rather viscous and astringent, and not at all pleasant." This last plant seems nearly allied to *L. tenuissimus* of Sir J. E. Smith's English Flora.—3. *Potamogeton compressum*.—This rare species of pond-weed grows in a small loch, in perfectly still water, quite erect, and generally about two feet high.—4. *Asplenium alternifolium*. This was originally observed by the late distinguished Mr Dickson of Covent Garden, growing on " sunny rocks two miles from Kelso," and no other botanist had ever gathered it: from an observation in Dr Hooker's Flora Scotica, that " in Switzerland it is quite an alpine species," it appears that Mr Dickson's accuracy was rather questioned; but it is now placed beyond a doubt.

32. *Ledum palustre and Papaver nudicaule.*—Our botanical readers will be not a little surprised to learn, that these plants, hitherto considered as peculiar almost to the Arctic Regions, now fall to be added to the British Flora. The credit of their discovery is due to Sir Charles Giesecké, who, in examining the mineralogy of the numerous small islands on the West Coast of Ireland, was delighted to meet with two old vegetable friends whose acquaintance he had made in Greenland, growing on the

high hills of the remote island of Achlin; the *Ledum palustre* in quantities together, in elevated marshy grounds; the *Papaver nudicaule* scattered in single plants, among rocky glens in the hills.

33. *Chara aspera.*—Mr Charles Clouston of Stromness Manse, Orkney, an assiduous cultivator of botanical science, has lately added *Chara aspera*, Willd. to the Flora of Great Britain. We have compared Mr Clouston's plant with authentic specimens received from Professor Agardh, and consider it as a well established species. The genus, it may be observed, is deserving of peculiar attention, on account of the very opposite opinions entertained of its affinities. Sir James Edward Smith continues to place it among the Phænogamous plants, in the first class of the Linnean system. Dr Hooker places it in Cryptogamia, between the *Algæ* and *Hepaticæ*. Agardh, in his recently published *Systema Algarum*, considers it as belonging to the true *Algæ*, and places it between the genera *Bulbochæte* and *Ceramium*. The latter author has divided the species into two genera, *Nitella* and *Chara*. In conclusion, we may notice, that Dr Hooker mentions, in his Flora Scotica, that M. Leman is of opinion the *Charæ* are allied to the "*Onagrariæ* and *Salicariæ*, and proposes, that the genus *Chara* should constitute a new family of Dicotyledons, under the name of *Eleodeæ*." M. Leman draws his conclusions from his examinations of the *nucule*, in the fossil state.—*R. K. G.*

ZOOLOGY.

34. *Sphinx Atropos.*—Mr Donovan, in his "British Insects," remarks, that this is nowhere common, and is rare in England; and adds, that he once met with the larva of full size, but it died. We may mention, as evincing the peculiar warmth and dryness of the past summer season in this part of Scotland, that the larvæ of this large sphinx appeared pretty common in the country around Edinburgh, during the month of August. They were generally found in potato fields, and feeding on the leaves of the potat [redacted] The great size of the caterpillars, which were from inches in length, and of proportional thickness, ted the notice of the country people,

and in some cases excited no little alarm. It may be added, that few specimens of the perfect moth have since been seen: it is therefore probable, that, owing to the occurrence of a good deal of wet weather in September, most of the larvæ had perished.

35. *An appearance seen on the Surface of the living Corallina officinalis.*—When a small living branch of the corallina officinalis is placed under the microscope with sea-water, we observe the rounded extremity of each of the last digitations tipt with a thin layer of a soft transparent colourless matter; this transparent covering is spread completely over the free ends of all the branches, is thickest in the centre, and tapers gradually to the sides, where no trace of it is seen; on the surface of this matter we can distinguish very minute tubercles or papillæ, likewise transparent, but which do not appear to have any motion. I have not observed this on any other part of the coralline; and as it appears to have escaped notice, and may possibly have some connection with the mode of growth of a substance whose nature is still perfectly unknown, I have thought it worthy of being suggested to the attention of zoologists..—*Dr Grant.*

36. *On the Spicula of the Spongilla friabilis,* Lamarck.—In the forms and combinations of the ultimate spicula, in their arrangement into groups, and in the disposition of these groups around the pores, canals, and fecal orifices of the sponge, we observe the same inexplicable uniformity of design in each of the different species, which is displayed in their outward forms, and in the disposition of their individual parts. This unity of plan is equally discernible in the structure of the *Spongilla friabilis,* which we have shewn in a separate memoir to bear the closest resemblance to that of the true sponge. There occurs but one form of spiculum in this species of spongilla; it is simple, curved, cylindrical, and acutely pointed at both ends; like most of the marine spicula, and the axes of many supposed keratophytes, it abounds with silica, and scratches glass, both in its natural and calcined state. Viewed through the microscope in its natural state it appears transparent, solid, and homogeneous throughout, but on being kept for a minute or two at a red heat, it loses its transparency and symmetrical form: it becomes distended like a

bottle in some part of its course, generally in the middle, sometimes near one end, and bursts without any audible decrepitation. In their white, opaque, calcined state, nitric acid, vinegar, or muriatic acid, produce no more effect on them than pure water. It is stated by Lamouroux that the burnt ashes of the spongilla abound so much with lime, that sometimes more than half of their weight is composed of that earth *; he has not mentioned, however, the species in which he met with this appearance, and may possibly have been deceived by portions of shells in its substance, or by small fragments of the calcareous rocks on which the animal grew. When the spicula are examined through the microscope after this exposure to heat, we distinctly perceive a shut cavity within them, extending from the one point to the other; and on the inflated part of each spiculum we observe a ragged opening, as if a portion had been driven out by the expansion of some contained fluid. In those spicula which had suffered little change of form by their incandescence, I have never failed to observe the same cavity within, extending from one end to the other, and a distinct open rent on their side, by which the contained matter has escaped before the usual globular distension had taken place. From the constancy of the form of this spiculum, in every variety of *Spongilla friabilis* I have met with in Lochend, whether lobed, branched, flat, or globular, grey coloured, or green, young, or old, I am convinced that it will afford an equally useful and scientific character for the discrimination of this animal, as that afforded by the spicula of the marine sponge, and ought, in like manner, to have a place in the definition of the species. This interesting character in the marine sponges has been neglected by Lamarck, and only partially adopted by Donati, Ellis, Gmelin, Montagu and Lamouroux. Although the spiculum above described occurs uncombined with any other form in this fresh-water species, and possesses nearly the simplest possible form, we almost always observe in the marine sponges a combination of more than one primitive form in the same individual; and these forms often very complicated, as in the tri-radiate and quadri-radiate spicula. The form of a single spiculum may be sufficient to distinguish the

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[illegible] Polyp. 1816, a-Ephydatia.

few known species of fresh-water sponge; but the form of one spiculum only, in the marine species, is of no value in characterising them, from the important circumstance of the same form not unfrequently occurring in different species. Thus the tri-radiate spiculum of the *Spongia compressa* will not suffice alone to distinguish that species from the *Spongia botryoides*, since Ellis, Gmelin, Montagu, and Lamouroux, have described and delineated the same form of spiculum as occurring in the latter species; but when we combine the compound tri-radiate spiculum of the *Spongia compressa* along with its only other simple, clavate, bent spiculum, we establish a scientific and permanent character, which will distinguish it from every existing species. These views, regarding the marine spicula, I had occasion, last winter, to illustrate in the Wernerian Society, and have since had an opportunity of extending them only to one species of the fresh-water sponge. Should the anomalous circumstance occur, of the same curved simple spiculum appearing in different species of spongilla, uncombined with any other form of spiculum, a specific difference must be sought for in the next character, pointed out by Mr Ellis in the *Spongia urens*, in the mode of arrangement of the spicula in the groups; and this character in the *Spongilla friabilis* consists in a remarkable parallelism of the spicula composing all the longitudinal fasciculi. The spicula of this animal are about half a line in length, and so slender as to be almost invisible to the naked eye; they have a shining vitreous lustre, and appear like the finest filaments of grey flexible asbestus; they do not appear to grow after they are once formed; for, when the ovum has newly fixed itself, and begun to spread on a watch-glass, I have constantly observed, that the spicula make their appearance in the transparent film of their full size, and with their symmetry complete; their lustre is not tarnished by remaining in nitric or muriatic acids; although the ovum is nourished only with rain-water, it continues to secrete these shut, flexible, siliceous tubes.—*Dr Grant.*

37. *Sounds produced under water by the Tritonia arborescens.*—About a month ago I happened to place together, in a crystal jar filled with sea-water, some small species of *Doris*, specimens of the minute *Tritonia coronata*, *Eolis peregrina*, and two of the *Tritonia arborescens*, and my attention was soon af-

ter excited by a clinking sound proceeding from the vessel. On separating these naked gasteropods into different vessels, I observed that the sounds were produced by the *Tritoniæ arborescentes*, and by them only. The sounds they produce, when in a glass vessel, resemble very much the clink of a steel-wire on the side of the jar, one stroke only being given at a time, and repeated at intervals of a minute or two; when placed in a large basin of water the sound is much obscured, and is like that of a watch, one stroke being repeated as before at intervals. The sound is longest and oftenest repeated when the *Tritoniæ* are lively and moving about, and is not heard when they are cold and without any motion; in the dark I have not observed any light emitted at the time of the stroke; no globule of air escapes to the surface of the water, nor is any ripple produced on the surface at the instant of the stroke; the sound, when in a glass-vessel, is mellow and distinct. I have kept these *Tritoniæ* alive on my writing table for a month, by renewing their water every other day, and giving them occasionally fresh branches of the *Sertularia dichotoma*, which they are very fond of creeping upon, and from which they seem to derive nourishment, by constantly squeezing its tender ramifications between their two teeth; and during the whole period of their confinement, they have continued to produce the sounds, with very little diminution of their original intensity. In a still apartment they are audible at the distance of twelve feet; they have been heard by several friends, and by the President and a few of the members of the Wernerian Society. The sounds obviously proceed from the mouth of the animal; and at the instant of the stroke we observe the lips suddenly separate, as if to allow the water to rush into a small vacuum formed within. As these animals are hermaphrodites, requiring mutual impregnation, the sounds may possibly be a means of communication between them; or if they be of an electric nature, they may be a means of defending from foreign enemies, one of the most delicate, defenceless, and beautiful gasterophods that inhabit the deep.—*Dr Grant.*

38. *Pecten niveus, a new species.*—It having been suggested, in ha [illegible] the number of the Annals of Philosophy for Nove [illegible] the *Pecten niveus*, described in vol. xiii. p. 166. o [illegible] ical Journal, is perhaps a mere variety

of *P. islandicus,* I judge it expedient to institute a comparison between the two species, after the manner in which I have compared *P. niveus* with *P. varius*, the *only* species to which it approaches in its characters. *P. islandicus* has from 70 to 100 or more [*] ribs; *P. niveus* has invariably 46 [†]; in the *former*, the ribs are very irregularly grouped, from 2 to 6 being crowded together, with smaller ones intervening, but without any regularity; in the *latter*, they are beautifully regular; in *P. islandicus*, they are marked with very numerous, delicate, erect laminæ, or scales, without any appearance of echinations; in *P. niveus*, they are compact and smooth, with scattered echinations toward the margin of the shells; *P. islandicus* is a tolerably thick shell, of a pale reddish colour, with concentric circles of a deeper tint; *P. niveus* is a very thin shell, of a pure white colour: *P. islandicus* has a margin singularly irregular in its teeth, recalling the idea of that sort of leaf which is termed *folium crispatum; P. niveus* has its marginal teeth as regular as those of a cockle. If, after this, *P. islandicus* and *P. niveus* should be considered identical, then assuredly, *P. maximus* and *P. jacobæus* are so also; and scarcely any two species of a genus can be named, that must not, on the same grounds, be mere varieties. I now subjoin the distinctive characters of the three species.—*P. islandicus*, testâ suborbiculari rubente, fasciis concentricis saturatioribus, radiis circiter 100 varie aggregatis rotundatis lamellulis densissimis scabriusculis. *P. niveus*, testâ orbiculari, fragili candidâ, radiis 46 subcompressis rotundatis sparsim breviter tenuiterque echinatis. *P. varius*, testâ orbiculato-oblongâ, colore variâ, radiis 32, obsolete squamosis, subcompressis, rotundato-planatis, sparsim crasse echinatis.—*W. McG.*

39. *Balls in the Stomach of Fishes.*—A globular substance is found on the shores of the Mediterranean, which has much resemblance to the balls of hair formed in the stomach of oxen, goats, and some wild animals, but which appears to be produced by an agglomeration of the leaves of *zostera marina* in the sto-

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[*] In a specimen in the Museum of the University of Edinburgh, the number is 104; in a very perfect specimen belonging to W. Nicol, Esq. Edinburgh, the number is 106.

[†] That is to say in 32 specimens.

mach of certain fishes. The people use them in many places on the coasts of Spain for keeping fire alive in the house. Before putting out the fire, which they may have been using for domestic purposes, they kindle one of these balls by applying it to a piece of burning coal, and then deposit it in a corner of the chimney. The fire spreads very slowly, so as not to consume the ball within less than twenty-four hours, by which means a light may be obtained at any time.—*Bullet. Univers. August* 1825.

40. *East Indian Unicorn.*—It having been asserted by the *Bhoteas*, that an animal, called by them the *Chirsee*, was the *Unicorn*, and the horns which they produced proving that they spoke of no imaginary creature, exertions were made, we are told in the Calcutta Oriental Magazine, to procure a specimen of the animal in question. Accordingly, the skin of one was sent to the resident at *Atamandra*, with the *horns* attached, shewing the animal to be no unicorn, but an antelope, of a species apparently new. There was no possibility of procuring it alive, as it frequents the most inaccessible part of the snowy mountains, among the haunts of the musk deer, and is exceedingly vigilant and easily alarmed. It is alleged, that although the animal produced has two horns, yet, that some individuals of the species have only one horn. The dimensions, so far as they could be taken from a shrivelled skin, were as follows: Total length 5 feet 8 inches; length of body 4 feet 2 inches; length of head 10 inches; length of horns 2 feet 1½ inches; tail 8 inches; ears 4½ inches. The colour is bluish grey, inclining to red, especially on the back; the hair, about an inch long, and resembling in structure that of the musk, with a mixture of very soft wool lying close to the skin. The forehead is nearly black, as well as the legs; the belly white; the snout whitish; the horns are placed very near each other, on the back of the head, and marked with annular prominences, which are most conspicuous on the upper side of the horn. The animal here imperfectly described, if a distinct species, will furnish an interesting addition to the very extensive family of antelopes; but, as Cuvier remarks, it is surprising to find men still persisting to search for what the established la [illegible] e nature demonstrate to be a physical impossibility [illegible] uminating animal, with a single horn placed up [illegible] ture. That the Chirsee should occa-

sionally have only one horn, we can very readily believe, because such an occurrence is not uncommon among antelopes, but it is not natural, being merely the effect of accident; and as the horns of this species are described as being very close upon each other, the loss of one of them might easily induce an ignorant person, who had seen or procured an animal so mutilated, to imagine it a true unicorn.

41. *Cause of the Red Colour of Lake Morat.*—Professor De Candolle of Geneva lately read to the Helvetic Society of Natural Science, a memoir upon the botanical nature of a reddish substance which was observed upon the surface of the lake of Morat last spring, and which has attracted the attention of the botanists and chemists of Geneva. This substance made its appearance in calm weather, and was disposed in large zones upon the edges of the lake, especially about the reeds. In the different parcels sent from Morat, there were found two distinct substances; 1*st*, A greenish fetid substance, leaving when it deposited the upper part of the water tinged with a red colour; 2*dly*, A lamellar substance in irregular shreds, of a soft and spongy consistence. The first of these substances, viewed through a powerful microscope, and minutely observed by MM. Vaucher, De Candolle and Prevost, had all the appearance of an oscillatoria. The observers even distinctly perceived the motion of this zoophyte, and the species to which it appeared to come nearest is the *Oscillatoria subfusca* of Vaucher. Compared with this latter, however, which M. Vaucher had himself taken at the edge of the Rhone, it presented sufficiently distinctive characters to constitute a new species. M. De Candolle has named it *O. purpurea.* The other substance submitted in the same manner to the microscope, presented no traces of organization, and no distinct idea could be formed of its nature. Whether it be a zoophyte of the same family as the last, or merely the remains of aquatic plants, it is impossible to decide, without a careful examination of it in the spot in which it occurs. The phenomenon which has given rise to these inquiries does not seem peculiar to the lake of Morat, but is equally observed in other lakes in Switzerland; and, it is said, that the fishermen have sometimes observed it at the upper part of the lake of Geneva. A warm and dry season, together with a low state of the water, are the cir-

cumstances most favourable to the development of the myriads of oscillatoriæ which redden the waters. Haller, and a preceding author, have already mentioned a conferva, which they distinguish by the same character, and which is perhaps identical with the oscillatoria of which we are speaking. M. Colladon of Geneva read a memoir, containing the results obtained from the chemical analysis of this substance. It was conducted by MM. Colladon, Peschier and Macaire, and agrees with the microscopical observations of MM. De Candolle, Vaucher and Prevost, in shewing that the substance in question is an oscillatoria. This analysis has discovered the following materials in its composition. 1*st*, A red colouring matter, partly soluble in alcohol. 2*d*, Chlorophylle. 3*d*, Gelatine in considerable quantity. 4*th*, Albumen. 5*th*, Some earthy and alkaline salts, and a little oxide of iron. These results confirm the opinion of some naturalists respecting the products of animal nature which are met with in a great number of mineral waters, and give support to the observations made by Vauquelin, upon the green substance of the waters of Vichy, in which he found a substance that had much resemblance to albumen.—*Bibliotheque Univers. August* 1825.

FOSSIL ZOOLOGY.

42. *Discovery of the Anaplotherium commune in the Isle of Wight.*—The identity of the fresh-water formations of the Isle of Wight, with those in the vicinity of Paris, has been clearly established, since the publication of Mr Webster's excellent memoirs on the former; and this conclusion has rested upon the similarity of the remains of fresh water mollusca and vegetables which these respective formations contain, and on the correspondence in their substance, and their relative position to other strata of marine origin, quite sufficient to place the contemporaneous deposition of these remarkable strata out of doubt. There still remained a point, however, on which evidence seemed desirable, inasmuch as the remains of the large quadrupeds which occur in the basin of Paris, had not been ascertained to exist in England. T [redacted]ideratum has been in some measure supplied, by Profess [redacted]* having lately discovered in the collection

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'hilosophy for November 1825.

of Mr Thomas Allan of Edinburgh, a tooth, which the latter gentleman had himself found several years ago in the quarries of Binstead, in the Isle of Wight, and which, with the assistance of Mr Pentland, has been ascertained to be a molar tooth of the lower jaw of the Anaplotherium commune.

43. *Petrified Fishes.*—Mr Sinclair of Ulbster, M. P. lately transmitted to Professor Jameson, for the College Museum, a collection of petrified fishes, found by him in the old red sandstone formation in the neighbourhood of Thurso; and the minister of South Ronaldshay, one of the Orkneys, lately deposited in the College Museum specimens of the same description, collected by himself in the old red sandstone of that island. These fishes are found in the variety of sandstone flag now so extensively imported into Edinburgh from Caithness, and which we first pointed out to the attention of builders and others many years ago.

ANTHROPOLOGY.

44. *On the causes of Bronchocele.*—The enlargement of the thyroid gland, called by medical men Bronchocele, and commonly known in England by the name of Derbyshire Neck, and in France by that of Goitre, is an endemical disease, or one that takes place only in certain districts. It is a complaint that occurs very frequently in Nottingham, and the surrounding country. The disease is to be met with, I believe, throughout Derbyshire, but in some places more commonly than in others. I was lately told, that there are not fewer than a hundred women in the village of Cromford, near Matlock, who labour under bronchocele of a large size. As to the cause of this disease, there are various opinions: the vulgar one here ascribes the disease to the hardness of the water, and, as far as I have had an opportunity of inquiring, the same opinion obtains in Derbyshire. This popular notion certainly receives confirmation from the circumstance, that Bronchocele is more frequently to be met with, and of a larger size, where the water in common use is very hard, than when it is of a softer quality. The water with which the inhabitants of Nottingham are chiefly supplied, is from the river Leen, that runs close to the town, and well-water. The Leen is chiefly surface water, and is forced by an engine into a

reservoir, from which it is conveyed in leaden pipes to the greater part of the town, and is certainly a soft water, and answers very well for washing, and all other domestic purposes. The well-water is more or less hard; the softest is brought from Sion Hill and New Radford water-works, in carts, to supply the inhabitants of those parts of the town that are not furnished with water by pipes from the reservoir. The well-water in the town is very hard, and unfit for domestic purposes, although many persons, I know, use it for drinking, brewing, and making tea, in preference to the river-water. Well-water is also very much employed by the inhabitants of the country round Nottingham, and some of the wells are very deep, particularly in the coal district, where they are often drained of their water by sinking deep shafts to get the coal. A respectable surgeon, who practises in the coal district, informs me, that bronchocele is more common now than it was in his younger days, and he ascribes it to the wells being sunk deeper than formerly, from the circumstance mentioned above. In certain districts of the Alps, bronchocele occurs so frequently and so generally, that it appears to be both hereditary and endemial; by some, the disease has been ascribed to elevated situation and low temperature; by others, to the use of snow or ice-water. If elevated situation and low temperature had any share in the production of the disease, we ought to meet with it every day in Sweden, Norway, and the Highlands of Scotland; but, so far from this being the case, the fact is, that the disease is unknown in those countries except by name. The late Dr Reeve of Norwich, who had travelled in Switzerland, and was familiar with bronchocele, observes, "with regard to the alleged causes of goitre, the general opinion of its being endemial in mountainous countries is of no value, because the disease is rare in Scotland, and very common in the county of Norfolk." That bronchocele is occasioned by something in the river or well water, used by persons residing in the district where the disease is endemic, and not by snow or ice-water, is, I think, proved beyond a doubt, by the following facts, recorded by Dr Richardson, who accompanied the late arduous expedition to th[illegible]n Polar Regions, under the command of Captain Fr[illegible]he Royal Navy. He says, "bronchocele or goitr[illegible]n disorder at Edmonstone. I examined several of the individuals afflicted with it, and endeavoured to obtain every information on the subject from the most authentic sources. The following facts may be depended upon: The disorder attacks those only who drink from the *water* of the *river*. It is indeed in its worst state, confined almost entirely to the half-breed of women and children who reside constantly at the fort, and make use of river-water, drawn in winter, through a hole made in the ice. The men, from being often from home, on journeys through the plain, where their drink is melted snow, are less affected: and if any of them exhibit, during the winter, some incipient symptoms of the complaint, the annual summer voyage to the sea-coast generally effects a cure. The natives, who confine themselves to *snow-water* in the winter, and drink of the small rivulets which flow through the plains in the summer, are exempt from the attacks of this disease." A residence of a single year at Edmonstone, is sufficient to render a family bronchocelous. Many of the goitres acquire great size. Burnt sponge has been tried, and found to remove the disease; but an exposure to the same cause immediately produces it. A great proportion of the children of women who have goitres, are born idiots, with large heads, and the other distinguishing marks of *cretins*. I could not learn whether it was necessary that both parents should have goitres to produce cretin children." I may here remark, that in no instance have I observed mental imbecility, or the disease called Cretinism, in the least connected with bronchocele, as it occurs in this part of the country. From what has been stated above, it is sufficiently clear, that *elevation* of situation, and *temperature* of the water, have nothing to do with the production of bronchocele. That it is occasioned by something in the water commonly used, in the place where the disease is endemic, is, I think, sufficiently proved by the extracts from Captain Franklin's Journal, given above. As to the particular substance in solution in the water, that occasions bronchocele, I freely confess my complete ignorance; but let us hope that this noxious matter will sooner or later be detected by some one gifted with superior talents for chemical research *."

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* The above notice is extracted from a valuable and important medical work, lately published, under the title, "*Medical Researches on the effects of Iodine in Bronchocele*," by Alexander Manson, M. D.

PHYSIOLOGY.

45. *Canals in the Filaments of the Nerves.*—Messrs Cuvier, Dumeril, Geoffroy St Hilaire, and Dupuytren, have been charged by the Academy of Sciences to examine the preparations made by M. Bogros, in reference to his discovery of canals in the filaments of which the nerves are composed, and to ascertain the existence of these canals, and of their true situation in the nervous tissue. M. Bogros will, without doubt, be impressed with the propriety of varying his injections and preparations in presence of the commissioners, so as to leave no doubt upon their mind. This point of anatomy is too important, and the commissioners are too well acquainted with anatomical researches, for their opinion regarding this discovery not to be definitive, and for their not determining with accuracy what may be perfectly ascertained, and what may still be doubtful in the matter. We shall make known the result of this investigation, so anxiously looked for by all anatomists.—*Bulletin Univers., Aug.* 1825.

46. *On the Iron in the Cruor, or red part of the Blood.*—Englehart of Göttingen, from a series of experiments, concludes, that the red colour of the cruor of the blood is owing to iron, although this opinion has been controverted by Brande, Vauquelin and others. He found, when the cruor is deprived of its iron, that it becomes colourless. The iron is separated from the cruor by means of chlorine, a method much superior to those at present in use.

STATISTICS.

47. *Prussian Universities.*—According to the Jahrbuch der Konigl Preussisch Universitaten, the number of students in 1821, at the Prussian Universities, was as follows:

At Berlin,	1,172
Bonn,	621
Halle-Wittemberg,	825
Breslau,	557
ifswald,	70
sberg,	218
	3463

ARTS.

48. *Manufacture of Paper from Marine Plants.*—It is said, that it has been tried with success in Holland to manufacture paper of marine algæ. We have not seen this paper, and are unable to say any thing with certainty upon the subject; but we entertain no doubts regarding the success of such an undertaking, provided it were conducted by proper hands. The tenacious texture, and the nature of these plants, seem to render them well adapted for this purpose.

49. *Spiritous Solution of Copal.*—From numerous experiments, the Sieurs Bravi and Wilhelm, distillers of spirits at Aschaffenbourg, have found out a spirit which possesses the faculty of dissolving copal without the aid of heat, and, in general, without any solvent vehicle. This spiritous solution of copal has a twofold advantage, inasmuch as it not only gives a shining lustre to articles of wood, horn, metal, pasteboard, &c. but also preserves this property in them, and insures them a permanent beauty, without even forming cracks, which is an inconvenience incident to every sort of varnish. It is employed like other varnish, being applied lightly to objects, by means of a pencil. It dries quickly; and very little is requisite to cover a pretty considerable surface. It is to be observed that this copal varnish does not admit of any mixture. It having been for a long time in use among artists, sufficiently attests its good qualities, and renders it unnecessary for us to recommend it. It is sold by the manufacturers themselves, in bottles and half bottles, at a very moderate price.

50. *Very strong Leather for Harness and other Saddlery work.*—In Poland and Russia, the twisted leather which they make themselves is preferred to every other kind for harness. For making this leather, dried cow-hide is taken; the hair is removed by means of boiling water, and a sort of scraper; it is then cut into long straps, which are sewed end to end; the two extremities of the long strap thus formed are then stitched together, and the strap thus becomes double. In this state it is impregnated with fatty substances made warm; it is then suspended by a hook to the roof, and weights attached to its lower

part. In this manner the strap forms two parallel bands, placed in a vertical position, and united above and below. Two sticks are passed between them, crossed horizontally; and they are turned round several times. By this means the two bands are twisted and pressed against each other as strongly as possible; and when the moving power ceases, they turn of themselves in the opposite direction. During this operation the leather is very sensibly heated; fatty substances are then applied to it anew, with which it is fully impregnated, and at length acquires an extraordinary degree of pliancy. The leather thus prepared lasts for a very long time, and preserves its good qualities in all sorts of weather.—*Bullet. Univers. Aug.* 1825.

51. *Composition for the Covering of Buildings, by M. Pew.*—The composition proposed by the author is destined to form a sort of unalterable and incombustible mastich. For this purpose, he takes the hardest and purest limestone that he can find, free from sand, clay, or other heterogeneous matter. White marble is to be preferred, if it can be procured. This limestone is calcined in a reverberatory furnace; it is then pulverised, and passed through a sieve. One part is taken by weight, and mixed with two parts of clay well baked, and similarly pulverised. This mixture must be made with great care. On the other hand, one part of calcined and pulverised sulphate of lime (gypsum) is taken, and two parts of clay, baked and pulverised, added to it. These two sorts of powder are then combined and incorporated, so as to produce a perfect mixture. The composition is preserved for use in a dry place, sheltered from the air, where it keeps for a long time, without losing its properties. When it is to be used it is mixed with about a fourth part of its weight of water, which is gradually added, stirring it continually, until it forms a thick paste. This paste is spread upon the laths and joists of buildings, which it renders entirely incombustible. It becomes in time as hard as stone; allows no moisture to penetrate, and is not cracked by heat. When well prepared it will last for any length of time. The composition when still in a plastic sta [redacted] eceive whatever colour it may be thought proper to giv

52. *M* *hod of rendering Gravers capable of Engravin* -Having been informed by his writing

engraver, that he should be obliged to give up the task of engraving upon steel-plates, owing to the impossibility of finding any gravers capable of cutting them, without perpetually breaking in the points, Mr Turrell hit upon the following method of accomplishing his object. He had formerly been much in the habit of seeing the singular manner in which the watch-spring makers in Clerkenwell treat the steel of which their springs are made. Pieces of steel-wire, of a proper quality and size, are spread by the hammer, when cold, into thin plates. After being brought to a certain thinness and width, they are hardened, and then tempered, over the flame of a spirit-lamp, to the spring-temper, or, as it is termed, the raven's grey colour; they are then subjected to the planishing and condensing action of the hammer, and being then brightened, are lastly *blued* over the flame of a spirit-lamp. Previous to their being *blued*, they had by the planishing, condensing, and polishing, apparently lost all their elasticity and hardness, and could be readily bent in any manner, and would afterwards remain so bent, as though they had never been hardened and tempered at all; and yet, upon being *blued*, they regained all that *elasticity* for which they are so highly esteemed Mr Turrell, considering the above circumstance, thought, that, upon tempering a graver, though not to the degree used by the watch-spring makers, it might possibly be rendered capable of being acted upon by the blows of a hammer, so as to condense the pores of the steel, opened, as they must be, by the heat necessary in even the most careful hardening, but still more in the usual manner of making gravers in great numbers. He therefore tempered a graver to the *straw-colour* only, and had the satisfaction to find, that, on laying the back of it upon a rounded anvil, he could, by a repetition of gentle blows, with the flat cross pane of a small and very hard cast-steel watchmaker's hammer, succeed in *rounding* or *blunting* the acute edge of the belly of it considerably, thus proving that it had undergone a great degree of condensation; and upon again tempering it to a straw-colour, and grinding and whetting the edge to a proper shape, the graver readily cut the steel-plate, and continued to do so, it being evidently also much toughened by this additional labour. He has since repeatedly succeeded in thus improving the quality of those Lancashire or Sheffield gravers which are to be met with in the tool-shops; and with such his writing engraver has now much less difficulty in performing his work than before. This process of Mr Turrell's, of *hammer-hardening* his gravers on the angular edges *cold*, may still admit of improvement. If the gravers were to be heated to the tempering degree, at the time of hammering them, the condensing effect of the hammer would be much greater. Mr Turrell finds, that, after hammering his gravers a certain time, they yield a sharp ringing sound to the blows, very different to that which they afforded on his beginning to hammer them; and that, after perceiving that sound, he does not find that the hammer exercises any further action upon them, in condensing them. Possibly a renewal of the heat may promote their further condensation.—*Gill's Technical Repository, Nov.* 1825.

53. *Excellent Building Stone near to Elgin.*—At a late meeting of the Directors of the Scottish National Mining Company, there were submitted to their attention, besides many interesting specimens of ores, &c., specimens of a sandstone from the Earl of Fyfe's quarries, near to Elgin. The colour is a yellowish-white, and the substance and texture of the stone good. It was considered, and with justice, as one of the most beautiful and excellent stones in the country, and well deserving the attention of those architects who wish to conjoin in their material richness and beauty of colour with durability of substance.

54. *Remarks on the Cultivation of the Silk-Worm, by John Murray, F. R. S. &c.*—This little work contains a condensed view of the facts communicated to the public in the Treatise of Count Dandolo. Mr Murray, in making known these important details, has it chiefly in view to invite our countrymen to introduce and cultivate silk in Great Britain. Those who are interested in this subject will find Mr Murray's Essay worthy of their attention.

55. *Manufacture of a Paper which has the property of removing Rust from articles of Iron and Steel.*—After having dried a certai[illegible]ntity of pumice among live coals, and reduced it to powde[illegible]th linseed oil varnish, and then dilute it with the s[illegible]til it be thin enough to be laid upon paper with [illegible] give this layer a yellow, black, or

brownish-red colour, the mass is mixed, before applying it to the paper, with a little ochre, English red, or lamp black. Care must be taken to lay the substance on as equally as possible, and to dry it in the air. When the first coat thus applied to the paper is dry, another is to be laid on in like manner. Those who manufacture it for sale pass the paper thus prepared under a cylinder, to render it smooth. It is further to be observed, that the mass must be liquid, and that it must be stirred about before applying it to the paper.

56. *On the Chinese manner of forming Artificial Pearls*, by E. GRAY, Esq.—" In a late visit to the College of Surgeons, I observed some pearls in the same species of shell *(Barbala plicata)*, which had the external appearance of being formed artificially, which Mr Clift, the excellent conservator of this establishment, very kindly allowed me to examine and describe. These pearls are of a very fine water, and nearly orbicular; their base is supported by a small process, which separates at the end into short diverging processes, which stand off at right angles to the central rib. On more minute examination, it appeared that these pearls were produced by there being introduced between the mantle of the animal (while yet alive) and the shell, a small piece of silver wire, bent into a peculiar form, that is to say, so as to form a right angle, with one arm ending in two diverging processes, so as to make the simple end always to keep its erect position. These wires must be introduced in the same manner as the semi-orbicular pieces of mother-of-pearl in the other method of forming artificial pearls, as there is no appearance of any external injury. The pearls are solid, and nearly orbicular, with a small pedicel, which is continued so as to entirely cover the wire. They may be perforated and used so as to show their whole surface, which I did not expect could ever be the case with any artificial pearls; but they must doubtless, unlike the artificial pearls formed by the other means, be a considerable time in coming to any useful and valuable size."—*Annals of Philosophy*, *November* 1825.

57. *Diving Bell.*—A patent has been obtained by Thomas Steel, Esq. A. M. of Magdalene College, Cambridge, for some very important improvements in the construction and appa-

ratus of the diving-bell. The improved bell will enable a directing engineer to descend, and remain at any depth at which diving-bells can be worked, without being subjected to endure the pressure of condensed air; and the working itself is rendered much more safe and effective, by means which Mr Steele has invented for communicating, by conversation, with those above, which will supersede the present imperfect and insecure system of signals by strokes of the hammer. He has further invented, by the application of optical principles, an instrument for the stronger illumination of objects under water; and improved the means of detaching men from the bell.

58. *Platina Strings for Musical Instruments.*—It was proposed some time ago, in the Musical Gazette of Leipsig, to employ platina strings instead of copper, steel, or brass ones. This metal being more elastic and more extensible than any other hitherto employed in the manufacture of strings, it is obvious that strings made of it would not only give a fuller sound, but would also have the advantage of keeping free of rust, and the inconvenience of breaking, as this metal is not influenced by humidity.—*Neues Kunst und Gewerbblatt, April* 1825.

59. *Imitation of Mahogany.*—When any white wood is frequently done over with a concentrated solution from shavings of mahogany, and then polished, it acquires a lustre and colour much resembling that of mahogany wood.

60. *Mode of securing Wooden Buildings from the effects of Fire.*—Two years ago the great theatre in Munich was burnt to the ground. This unfortunate accident roused the attention of the chemists of Bavaria to endeavour to discover some means of destroying the inflammability of wood; and of all the methods, the best, and that which has been employed in the new theatre just finished, was invented and proposed by Dr Fuchs, Professor of Mineralogy in Munich. The following is the process: 10 parts of potash or soda, 15 parts of quartz (sand), and 1 part charcoal, are melted together. This mass dissolved in water, and, either [illegible] mixed with earthy matters, applied to wood, completely [illegible]om the action of fire. The detailed account of [illegible] be given afterwards. As the mate-

rials, viz. the alkali, quartz, and charcoal, are in plenty in most districts where houses are built of wood, the compound can always be had in abundance and at a cheap rate. In America, where dreadful fires are of too frequent occurrence, the preservative materials are abundant; and there we may expect to hear of the compound being extensively used.

61. *Table shewing the Quantity of Metallic Copper produced in England, Scotland, and Ireland, from* 1818 *to* 1822.

	1818.	1819.	1820.	1821.	1822.
Cornwall, - [*Tons Fr.*	6714	7214	7364	8163	9331
Devonshire, - - -	438	433	417	483	537
Staffordshire (Ecton), -	200	180	236	110	38
Anglesea, - - -	633	564	561	604	738
Other parts of Wales, -	90	60	40	39	55
Somersetshire, - -	...	...	3	28	...
Cumberland and Westmoreland,	...	...	20	18	21
Ireland, - - - - -	120	116	174	257	738
Scotland, - - -	...	...	5	12	11
	8195	8567	8820	9714	114,69

ART. XXXII.—*List of Patents sealed in England from October* 6. *to November* 17. 1825.

Oct. 6. To J. MARTINEAU junior and H. W. SMITH, London, for "Improvements in the manufacture of Steel."—Six months to enrol specification.

To Sir G. CAYLEY, Bart. for "a new Locomotive apparatus."

To J. S. BROADWOOD, London, for "Improvements in Square Piano-fortes."

13. To T. HOWARD, London, for "a Vapour Engine."

To N. KIMBALL, London, for "a process for converting Cast-Iron into Steel."

To B. SANDERS, Worcester, for "Improvements in making Buttons."

To J. DWYER, Dublin, for "Improvements in making Buttons."

13. To J. CLESILD DANIEL of Stoke, Wilts, for "Improvements in machinery applicable to the weaving of Woollen Cloth."

To J. EASTON of Heal Cottage, Bradford, Somerset, for "Improvements in Locomotive or Steam-Carriages, and in the construction of Roads for them."

21. To WILLIAM HIRST, L. WOOD and J. ROGERSON, Leeds, for "Improvements in machinery for raising and dressing Cloth."

Oct. 21. To R. S. Perumberton and J. Morgan of Llanelly, Carmarthen, for "a consolidated or combined Drawing and Forcing Pump."

To G. Gurney, London, for "Improvements in the apparatus for raising or generating Steam."

To L. W. Wright, Lambeth, for "an Improvement in the construction of Steam-Engines."

22. To H. C. Jennings, London, for "Improvements in the process of refining Sugar."

28. To Thomas Steel, Esq. of Magdalene College, Cambridge, for "Improvements in the construction of Diving-Bells, or apparatus for diving under water."

Nov. 1. To J. and S. Seaward, London, engineers, for "a new or improved method or methods of propelling Boats, Craft, and all kinds of Vessels, on canals, rivers, and other shallow waters."

To W. Raynard, Surrey, for "a circumvolution Brush and Handle."

To Vernon Royle, Manchester, for "Improvements in the machinery for cleaning and spinning of Silk."

To J. Isaac Hawkins, Middlesex, engineer, for "Improvements on certain implements, machines or apparatus, used in the manufacturing or preserving of Books, whether bound or unbound."

To J. and W. Ridgway, for "an improved Cock-tap or Valve for drawing off liquors."

7. To T. Seaton, Bermondsey, Surrey, for "Improvements on Wheeled Carriages."

To G. Hunter, Esq. late clothier in Edinburgh, for "an improvement in the construction, use, and application of Wheels."

8. To T. Shaw Brandreth of Liverpool, Esq. for "an improved mode of constructing Wheel-Carriages."

To Samuel Brown, gentleman, Middlesex, for "Improvements in machinery for making or manufacturing Casks and other vessels."

To W. E. Cochrane, London, for "an improvement in Cooking Apparatus."

To J. W. Hiort, Office of Works, Whitehall, London, for "an improved Chimney or Flue, for domestic and other purposes."

To C. Louis Girond of Lyons, in France, for "a chemical substitute for Gall-Nuts, in all the different branches of the arts or manufactures in which Gall-Nuts have been accustomed or may hereafter be used."

To James Winks and J. Erroyd of Rochdale, Lancashire, for "an Engine for cutting Nails, Sprigs and Sparables, on an improved system."

Nov. 10. To J. and A. Maccarthy, London, for "new and improved Pavement, Pitching, or Covering for streets, roads, ways, and places."

To B. Cook of Birmingham, for "a new method of rendering Ships' Cables and Anchors more secure, and less liable to strain and inju[illegible] vessel lies at anchor."

To B[illegible]ngham, for "Improvements in the Binding of Bo[illegible] of various descriptions."

Nov. 10. To J. G. Deyerlein, Middlesex, for "Improvements on Weighing-Machines, which machines he denominates German Weigh-Bridges."

12. To W. Francis Hamilton, Surrey, engineer, for "certain Alloys, or a certain Alloy of Metals."

17. To E. Bowring, London, and R. Stamp, Sussex, for "Improvements in the working, weaving, or preparing Silk, and other fibrous materials, used in making hats, bonnets, shawls," &c.

To J. Guestier, London, for "a mode or modes of making Paper from certain substances, which are thereby applicable to that purpose."

To A. Lamb, gentleman, London, and William Suttill, Middlesex, for "Improvements in machinery for preparing, drawing, roving, and spinning Flax, Hemp, and Waste Silk."

To G. Borradaile, London, merchant, for "an improved method of making or setting up Hats, or Hat Bodies."

Art. XXXIII.—*List of Patents granted in Scotland from 5th September to 17th November* 1825.

Sept. 5. To Joseph Alexander Taylor of London, gentleman, for "a new Polishing Apparatus for household purposes."

16. To Thomas Worthington junior and John Mulliner, both of Manchester, in the county of Lancaster, small-ware manufacturers, for "an Improvement in the Loom or machine used for the purpose of weaving or manufacturing of Tape, and such other articles to which the said loom or machine may be applicable."

17. To Charles Powell of Rockfield, county of Monmouth, gentleman, for "an Improved Blowing-machine."

21. To William Henry James of Cobourg Place, Winson Green, near Birmingham, county of Warwick, engineer, for "certain Improvements in the construction of Rail-Roads and Carriages."

30. To Benjamin Sanders of Broomsgrove, county of Worcester, button manufacturer, for "certain Improvements in the construction or making of Buttons."

Oct. 1. To Adam Eve of Louth, county of Lincoln, carpet-manufacturer, for "certain Improvements in manufacturing Carpets, which he intends to denominate Prince's Patent Union Carpet."

To Hugh Martin and Thomas Lee, manufacturers at Barrhead, parish of Neilstoun, county of Renfrew, for "an Addition and Improvement upon a Machine which was some time ago invented by themselves, for working by the hand a description of cloth made of cotton, and commonly called Fancy Net, in imitation of the French Net, or to be made of silk, woollen, and linen, or a combination of these, or part of these; and which machine, by means of this addition or improvement, can be wrought in a similar manner;

to the ordinary power-loom, by the application of steam, or other mechanical powers."

Oct. 4. To **James Wilks** of Rochdale, county palatine of Lancaster, tin-plate worker, and **John Ecroyd** of the same place, grocer and tallow chandler, for "an Engine for cutting Nails, Sprigs, and Sparables, on an improved system."

10. To **George Thompson** of Wolverhampton, county of Stafford, gent. for "an Improvement in the construction of Riding Saddles."

To **George Hunter** of the city of Edinburgh, late clothier to his Majesty, for "an Improvement in the construction, use, and application of Wheels."

To **Samuel Bagshaw** of Newcastle-under-Lyne, gentleman, for "a new Method of manufacturing Pipes for the conveyance of water."

13. To **Nathaniel Kimball** of New York, now residing in London, merchant, for "a process of converting Iron into Steel."

13. To **John Martineau** the younger, City Road, and **Henry William Smith** of Lawrence Pountney Place, London, Esq. for "certain Improvements in the manufacture of Steel."

To **Thomas Dwyer** of Lower Bridge Street, parish Dublin, for "certain Improvements in the manufacture of Buttons."

16. To **John Reedhead** of Heworth, county of Devon, gentleman, for "certain Improvements in Machinery for propelling Vessels of all descriptions, both in marine and inland navigation."

28. To **Henry Constantine Jennings**, London, practical chemist, for "certain Improvements in the process of Refining Sugar."

Nov. 5. To **Thomas Steele**, Master of Arts of Magdalene College, Cambridge, Esq. for "certain Improvements in the construction of Diving Bells or apparatus for diving under water."

To **John Bowler** of Nelson Square, Blackfriars Road, county of Surrey, and **Thomas Galon** of the Strand, London, hat-manufacturers, for "certain Improvements in the construction of Hats."

15. To **William Jefferies** of No. 46. London Street, Radcliffe Cross, parish of Radcliffe, county of Middlesex, brass-manufacturers, for "a Machine for Impelling Power without the aid of fire, water, air, steam, gas, or weight."

17. To **John Phillips Beavan** of Clifford Street, county of Middlesex, gentleman, for "a Cement for Building and other purposes, communicated to him by a stranger residing abroad."

Omitted at p. 151.......	Mean Temp. of September,	57°.260
	Mean Pressure, - -	29.472 inches.
——— at p. 154.......	Mean Temp. of November,	39°.850
	Mean Pressure, - -	29.482 inches.

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THE

EDINBURGH

PHILOSOPHICAL JOURNAL.

Art. I.—*The Geological Deluge, as interpreted by Baron Cuvier and Professor Buckland, inconsistent with the testimony of Moses and the Phenomena of Nature.* By the Rev. John Fleming, D. D., F. R. S. E. (Communicated by the Author.)

The science of Geology was first introduced to public notice, in this country, by philosophers who, while they cherished a reverential regard for the authority of the Scriptures, overlooked those methods of investigation which lead to a discovery of the laws of nature. Assuming that the first principles of geology were revealed to Moses, and communicated in the Book of Genesis, they were satisfied with a comparison of the scanty notices there given of the history of the Earth with the phenomena presented by its surface, even when the character and relation of these phenomena remained in a state of comparative obscurity. The original condition of the materials with which the Creator formed this Globe, long occupied the attention of those early cosmogonists; and, as the history of Moses was too meagre in its details to serve their purpose, and the Earth failed to exhibit the suitable documents, the imagination was called upon to supply that which neither the words nor the works of the Deity furnished. These reveries, however, usually termed Theories of the Earth, do not call for any comment at present.

The cause by which the deluge was produced, and the changes which it effected on the appearance of the globe, occupied the second place in the estimation of these geologists. Here, again, the details of Revelation were so deficient as to lead some to suppose that our copy of the Book of Genesis was more abridged than the one possessed by the ancient Jews.—(Kirwan's *Geol. Es.* 48.). The surface of the Earth was hastily looked at for proofs of the effects of this catastrophe; and again the imagination supplied that which observation could not yield. Burnet brought the waters from below, through the broken *crust* with which he fancied they had been covered during the antediluvian period, and with the fragments of this crust he formed the mountains. Woodward suspended, for a time, all *cohesion* among the particles of earth, and reduced the globe to a soft paste; while Whiston, not inferior in fancy to any of his predecessors, called a *comet* to his aid.

While philosophers were thus claiming the attention of the public in favour of their efforts to reconcile geology with revelation, they were powerfully assisted by individuals of another description. The "Place of Descent" where the "Ark" rested, had long been regarded as determined; remains of the timber had been preserved; and many pieces of the bitumen, with which it was calked, had been carried off to be employed as amulets for averting mischief. The skeletons of the antediluvian inhabitants were eagerly sought after; and the Continent of Europe seemed to furnish the expected documents:—even the grinders and thigh-bones of the antediluvian giants were disinterred from those graves which for so many ages they had occupied.

As science advanced, these theories of the deluge appeared in their true light; as unsupported by the statements in Scripture, and as inconsistent with the phenomena of nature. The skeleton of the antediluvian man became that of an acknowledged reptile; while the grinders and thigh-bones of the giants were admitted to belong to elephants. The geologist beheld his theories vanish like a dream, and the admirer of revelation felt (though very unnecessarily) as if a pillar of his faith had become a bro' [illegible] ed. Geology, by these premature attempts at generaliza [illegible] discredit as a science among philosophers, an [illegible] stian it was viewed with suspicion. The form [illegible] opinions and assertions substituted for facts; [illegible] d reaped the fruits of misplaced confidence. The friend of revelation had begun to consider the history of the deluge as the least perfect of those records which Moses has transmitted, since no proofs could be found in nature to attest the occurrence of the catastrophe. Need we be surprised, therefore, that a considerable degree of anxiety should prevail on this subject, with the religious public, and that any fresh attempt to revive their hopes would meet with a cordial, I had almost said a credulous, welcome? The truth of this view has been put to the test.

Baron Cuvier, so deservedly celebrated as a comparative anatomist, having devoted much labour to the investigation of fossil bones, naturally directed some portion of his attention to those collateral subjects which might serve to illustrate their history. In the preliminary discourse to his great work on "Fossil Bones," he announced the important results to which his labours, reading, and reflection had conducted him. This discourse was published in Edinburgh, in 1813, under the title of "Cuvier's Theory of the Earth." It has gone through several editions, and still continues to be a favourite with the public. It has contributed, in a very great degree, to render the study of geology popular in this country. How far the explanations which it offers of the phenomena of nature are true, and how far they are consistent with the sacred writings, will afterwards be considered.

The Reverend William Buckland, while Professor of Mineralogy in Oxford, appears to have embraced Baron Cuvier's views respecting the deluge; and, under their influence, distributed the modern strata (exclusive of the volcanic) into Postdiluvian detritus and Diluvian detritus.—(*Phil. Geol. England and Wales,* 1818.) In his "Inaugural Lecture," which was delivered May 15. 1819, before the University of Oxford, on the endowment of a readership in geology, he selected for the title, "Vindiciæ Geologicæ; or the Connection of Geology with Revelation explained:" and stated in the dedication, "that the facts developed by it (geology) are consistent with the accounts of the creation and deluge recorded in the Mosaic writings." In his subsequent inquiries, this learned and indefatigable Professor, who has contributed so much to exalt the geological character of England, has not only investigated the history of those beds of gravel and clay which contain fossil bones, but has successfully explored many caves which he considers as having been the *dens* of antediluvian animals. The results of these inquiries he has published in his "Reliquiæ Diluvianæ," in which he considers geology as "attesting the action of an universal deluge." This work, like the "Theory" of Cuvier, has greatly contributed to render the science of geology popular, by bringing it into favour with the Church, and even securing the countenance of the drawing-room. The general reader has been charmed with the novel scenes which it discloses, while the Christian has hailed it with joy, as offering a valuable testimonial to the authority of revelation.

To my "Remarks illustrative of the influence of Society on the Distribution of British Animals," inserted in No. XXII. of this Journal, Professor Buckland has conceived it necessary to make a "Reply," which has a place in No. XXIV. In this communication, he continues to advocate the opinions which he had advanced in the "Reliquiæ Diluvianæ," and attempts to obviate some of the objections which had been, incidentally, stated against them.

In an inquiry of this kind, regarded as highly interesting to the philosopher and the Christian, it seems requisite to exercise extreme caution. The fate of former theories in geology, which professed to explain the phenomena of nature, and to strengthen the authority of revelation, but which inquiry demonstrated to be visionary, intimates the risk of error, and calls for a minute examination of the value of the proofs adduced. I enter upon this inquiry as one deeply interested in the authority of revelation, and not indifferent to the progress of geological science. My remarks may not appear convincing, but they may excite that inquiry and discussion which lead to truth. It is impossible, however, in a paper of this kind, to enter into all the details which the general reader would probably desire. The outlines only of the subject can be noticed.

In reference to this important subject, two questions naturally occur to the mind:—1. Does the character of any of the members of tl [redacted] ata demonstrate the occurrence of a universal flc ely the agent in their formation?—2. Does the geological deluge, as supposed to be indicated by the phenomena of nature, agree with the character of the deluge of Noah, given by Moses?

In the following observations, I shall reverse this order of inquiry, for if the second question can be satisfactorily disposed of in the negative, it will leave the first to be examined entirely by the laws of physics, and in the absence of those *prejudices* which have been excited in the public mind on the subject. As a proof that such prejudices do exist, I may state that I have heard a gentleman of rank and piety, characterize the opposers of the diluvian hypothesis as embracing "*the infidel side of the question*;" and this, too, in the presence of the president and secretaries of the Geological Society of London. It is my object, in the present communication, to point out the infidel side of he question, viz. the one where error prevails. Nature, misinerpreted, may amuse the cosmogonist, but never can befriend the Christian. That which is true in science can alone give useful support to revelation; and that which is true in science never can be found opposed to its interests.

Does the character of the GEOLOGICAL DELUGE, *as supposed to be indicated by the phenomena of nature, agree with the character given of the* DELUGE OF NOAH, *by Moses?*

Before proceeding to state some of those points of difference between the two deluges, which appear to exist, I feel it to be necessary to notice one opinion which Baron Cuvier expresses without reserve. After intimating that "Moses and his people came out of Egypt," (Cuvier's Theory, p. 147.), he adds, "The legislator of the Jews could have no motive for shortening the duration of the nations, and would even have disgraced himself in the estimation of his own, if he had promulgated a history of the human race contradictory to that which they must have *learned by tradition in Egypt*. We may therefore conclude, that the Egyptians had, at this time, no other notions respecting the antiquity of the human race than are contained in the Book of Genesis." It is true, that Moses and his people came out of Egypt; but it is equally true that their fathers went into Egypt. Where, then, is the proof, that the history of the creation and the deluge, as given by Moses, was derived from the traditions of the Egyptians? Will the friend of revelation consider himself as under obligations to Baron Cuvier for this discovery? Or will the student of moral science admit its truth? Those individuals, in Britain, who cherish the highest respect for the authority of revelation, consider the information which Moses communicates as having been derived from a higher source than Egyptian tradition; and even the author of the strange remark acknowledges (p. 149.), that the Egyptians themselves had forgotten, for a long period, the tradition, "as we do not find any traces of it in the most ancient remaining fragments from that country. All of these, indeed, are posterior to the devastations committed by Cambyses." But where is the proof that the Egyptians possessed those traditions which the Jewish legislator has recorded, a thousand years before any traces of them occur in the monuments of their country, except the very inadequate one, "that Moses and his people came out of Egypt!" The cultivator of moral science, whose attention has long been arrested by the purity of the theism of the Jews, will naturally inquire, If Moses obtained all his knowledge of the creation and the deluge from the opinions or traditions of the Egyptians, may he not have derived his knowledge of the moral law from the same source? And may not the inquirer infer, that the prohibitory statutes against idolatry were forgotten by the Egyptians (and continue to be so), as had happened to them with respect to their traditions of the deluge, immediately after they had succeeded in impressing on the mind of the Jewish legislator a correct idea of their importance!

To such results, in my opinion, would Baron Cuvier's views legitimately lead. Nor, in the last edition of his great work, does he treat the authority of Moses with higher respect, since he considers the book of Genesis, as consisting of the shreds of former works, or, to use his own words, "Il suffit de la lire pour s'apercevoir qu'elle a été composé en partie avec des morceaux d'ouvrages anterieurs."—1. lxxxi.

Having made these preliminary remarks, I now proceed to point out those differences of character which appear to exist between the geological and Noachian deluges, and which prevent us from in[illegible] their identity.

1. The [illegible] luge, as interpreted by Baron Cuvier, was of su[illegible] permit the escape of different races of men by d[illegible] The Mongolian and Caucasian races

are so different in appearance from each other, " that one is almost tempted to suspect, that their ancestors and ours had escaped from the last grand catastrophe at two different sides." In reference to the Negroes, he states a similar opinion with less hesitation: " The circumstances of their character clearly evince, that they also have escaped from the last grand catastrophe, perhaps by another route than the races of the Caucasian and Altaic chains, from whom, perhaps, they may have been long separated before the epoch of that catastrophe." On the supposition that the different races of men were derived from a common stock, an idea sanctioned by revelation, supported by the truths of zoology, and tacitly admitted by our author, it seems difficult to discover any proof of their separation having been antediluvian. According to Moses, all that escaped of the human race, were eight individuals of the family of Noah. Here, then, we have the character of the geological deluge, in reference to the human race, as interpreted by Baron Cuvier, standing opposed to the history of the deluge as given by Moses, and that, too, in its most important feature.

2. The geological deluge, as interpreted by Baron Cuvier and Professor Buckland, occasioned the destruction of all the individuals of many species of quadrupeds. As examples of those which have thus suffered extinction, may be quoted, the fossil elephant, fossil hippopotamus, fossil rhinoceros, fossil bear, and fossil hyæna, besides many others. These have been, somewhat presumptuously, termed ANTEDILUVIAN ANIMALS [*]. In the history

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[*] In my first paper, in No. xxii. of this Journal, I have stated that the relics of these ancient animals occur in postdiluvian strata. The learned Professor, in his " Reply," first declares, " That, could the above cases be established, they would be decisive in favour of the theory maintained by Dr Fleming;" and shortly after adds, that, " Even admitting all these facts, still every atom of the evidence contained in my Reliquiæ Diluvianæ would remain unaffected by the discovery." I attempt not to reconcile such apparent contradictions. Perhaps it may be judged reasonable to allow an adversary, when hard pushed, to shift his position, even though it put the pursuer to more trouble. With reference to the Rhinoceros horn from Forfar, about which Professor Buckland is unnecessarily prolix, I may state, that I relied on the authority of Professor Jameson, in the Wern. Mem. iv. p. 582.; and having seen the horn labelled, as from Forfar, in the Edinburgh Museum, of which he is Regius Keeper, I still consider the statement of Professor Jameson to be substantially true, and the one given by my opponent as quite

of the Noachian deluge, as given by Moses, it is expressly stated, that clean and unclean beasts, fowls after their kind, cattle after their kind, and every creeping thing of the earth, two of every sort, male and female, were taken into the ark, preserved in the ark, and brought forth in safety from the ark, and dismissed with the mandate of their Creator to breed abundantly on the earth, and to be fruitful and multiply upon the earth. Here, then, we have revelation, declaring that, of *all species* of quadrupeds a male and female were spared and preserved during the deluge; while we have the phenomena of nature, as interpreted by the geologists we have quoted, intimating, that *all the individuals, of many species*, were not spared, not preserved, but *annihilated*, by the catastrophe. An error must exist in one of these statements. The declaration of Moses is positive. The phenomena of nature may not have been suitably investigated. Shall we reject, then, the conclusions of the geologist, and respect the authority of Moses, or give the preference to Cuvier and Buckland?

3. According to Baron Cuvier, " this revolution had buried all the countries which were before inhabited by men, and by the other animals that are now best known; and the same revolution had laid dry the bed of the last ocean, which now forms all the countries at present inhabited." (Theory, p. 171.) Moses expressly tells us, that the flood of waters was upon the earth, prevailing exceedingly upon the earth, and covering the highest hills; that the waters returned from off the earth. Here, again, we have the opinion of Cuvier, in direct opposition to the whole tenor of the history of the Noachian deluge. Nor need we be surprised at this, since he seems to be in opposition to himself. At one time he supposes, that the inundation did not reach to

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the reverse. The bottom of the horn attests its origin,—the numerous rents and their marly contents. The Blair-Drummond example I quoted from the same authority. It is singular, that, in the same number of the Journal in which this case is likewise treated as spurious, and in the Proceedings of the Wernerian Society, I found " Notices regarding the Rhinoceros Horns of Blair-Drummond, tending to shew that they may probably be regarded as having occurred in the blue clay of that district; by Mr A. B. Blackadder, Allan Park," p. 401. [redacted] Buckland has admitted, in his " Reply," my first example of *ex* *postdiluvian*, I have got quite enough to establish my view ed postdiluvian character of the gigantic elk is as decisive inoceros in a marl bed, or carcase of a mammoth in a post the summits of the higher mountain chains; and that Mongols, Caucasians, and Negroes may have escaped by different sides, or by different routes; at another, that the bed of the antediluvian ocean is now the abode of the post-diluvian quadrupeds.

4. The geological deluge, as interpreted by Professor Buckland, was sudden, transient, universal, simultaneous, rushing with an overwhelming impetuosity, infinitely more powerful than the most violent waterspouts. In the history of the Noachian deluge by Moses, there is not a term employed which indicates any one of the characters, except universality, attributed to the geological deluge. On the contrary, the flood neither approached nor retired suddenly. The waters rose upon the earth, during the continuance of the rain, for forty days; and they retired slowly, upon the rain being restrained. There is no notice taken of the furious movements of the waters, which must have driven the ark violently to and fro. On the contrary, there is reason to believe, from the writings of Moses, that the ark had not drifted far from the spot where it was at first lifted up, and that it grounded at no great distance from the same spot.

5. The geological deluge, as interpreted by Professor Buckland, excavated, in its fury, deep valleys, tearing up portions of the solid rock, and transporting to a distance the wreck which it had produced. On this supposition, the aspect of the antediluvian world must have been widely different from the present; lakes, and valleys, and seas, now existing in places formerly occupied by rocks, and the courses of rivers greatly altered. In the Book of Genesis there is no such change hinted at. On the contrary, the countries and rivers which existed before the flood, do not appear, from any thing said in the Scriptures, to have experienced any change in consequence of that event. But if the supposed impetuous torrent excavated valleys, and transported masses of rocks to a distance from their original repositories, then must the soil have been swept from off the earth, to the destruction of the vegetable tribes. Moses does not record such an occurrence. On the contrary, in his history of the dove and the olive-leaf plucked off, he furnishes a proof that the flood was not so violent in its motions as to disturb the soil, nor to overturn the trees which it supported; nor was the

ground rendered, by the catastrophe, unfit for the cultivation of the vine.

Viewing, in connection, these differences between the Mosaic history and these interpreters of the phenomena of nature, it seems impossible to admit, that, " as far as it goes, the Mosaic account is in perfect harmony with the discoveries of modern science." The reverse appears rather to be the case. It is well known, that Linnæus declared that he saw no examples in nature of the ravages of a universal flood : " *Cataclysmi* universalis certa rudera ego nondum attigi, quousque penetravi ; minus etiam veram *terram Adamiticam ;* sed ubique vidi factas ex æquore terras, et in his mera *rudera* longinque sensim præterlapsi ævi," (Syst. Nat. iii. 5.) ; and this opinion has given offence to several well disposed friends of revelation, who have, nevertheless, formed their notions of the deluge from the speculations of geologists, instead of the records of Scripture. I confess that I entertain the same opinion as Linnæus on this subject ; nor do I feel, though a clergyman, the slightest reason to conceal my sentiments, though they are opposed to the prejudices which a false philosophy has generated in the public mind. I have formed my notions of the Noachian deluge, not from Ovid, but from the Bible. There the simple narrative of Moses permits me to believe, that the waters rose upon the earth by degrees, and returned by degrees ; that means were employed by the Author of the calamity to preserve pairs of the land animals ; that the flood exhibited no violent impetuosity, neither displacing the soil, nor the vegetable tribes which it supported, nor rendering the ground unfit for the cultivation of the vine. With this conviction in my mind, I am not prepared to witness *in nature* any remaining *marks* of the catastrophe, and I feel my respect for the authority of revelation heightened, when I see on the present surface no memorials of the event. On the other hand, had I witnessed every valley and gravel-bed, nay, every fossil bone, attesting the ravages of the dreadful scene, I would have been puzzled to account for the unexpected difficulties ; and might have been induced to qu [illegible] the accuracy of Moses as an historian, or the claims of th [illegible] nesis to occupy its present place in the sacred reco [illegible] finding the Deity setting his bow in the cloud, [illegible] he would not again visit the earth with a flood, and as the *only natural token* of what had happened; I had expected to find a reference made to every diluvian heap of gravel, and every valley of denudation, as a memorial of that wrath which was displayed, while visiting rebellion with death. In other words, if the geological creeds of Baron Cuvier and Professor Buckland be established, as true in science, then must the Book of Genesis be blotted out of the records of inspiration. But as I believe in the authority of the Mosaic history, and see, in the opinion of Linnæus, a strict conformity therewith, in letter and spirit, I may perhaps be asked, How can I reconcile the phenomena of nature, as interpreted by these geologists, with the view which I have embraced? I have already, in my first paper, declared, that "the works and the words of God must give consistent indications of his government, provided they be interpreted truly." It has been announced, that the Mosaic account is in *perfect harmony* with the discoveries of modern science, though we have pointed out a *palpable disagreement*. Perhaps a similar difference may exist between these supposed discoveries of modern science and the phenomena of nature. Our attention will now be directed to the determination of this important point, involved in the second question we proposed to discuss. As now to be examined, it is one exclusively of a scientific character, in which all our appeals must be made to the facts established by observation or experiment.

II. *Does the character of any of the members of the "Modern Strata," demonstrate the occurrence of a Universal Flood as, exclusively, the agent in their formation?*

The progress of truth, in this branch of the inquiry, must necessarily be correlative with our knowledge of the "modern strata," and the causes which have operated in their production. Whether a sufficient degree of knowledge has been acquired, or sufficient attention been bestowed on the subject by British geologists, I leave to the determination of the unprejudiced. Enough, in my opinion, seems to have been secured to enable us to solve the question under consideration.

Various conjectures have been offered by different geologists, respecting the origin of the waters of the deluge. Some are disposed to consider the waters of the earth as sufficient, if once set in furious motion. A few look to a sudden change in the Earth's axis as the origin of the catastrophe, in the absence of all proof from the science of Astronomy. Some consider the waters as having been set in motion by the attractive force of a comet, without previously gaining an affirmative answer to the question, Has a comet this attractive force? There is abundant proof that the planets disturb the comets, but the converse is not known. The comet of 1454 eclipsed the Moon; while that of 1770 not only came near the Earth, but passed through the midst of the satellites of Jupiter, without producing any sensible effects. Others, translating the phrase of Moses, "the windows of Heaven," as literally meaning "a comet's tail," have considered the water as added to the Earth. I would be disposed, before admitting this view of the matter, to ask, Is the *vapour* of a comet's tail *aqueous*?—The following phenomena, however, bear more directly upon the question under discussion.

1. *Excavation of Valleys.*—Valleys, in the opinion of the supporters of the diluvian hypothesis, may have been produced by different causes, such as irregularity of deposition, or subsequent dislocations of the strata. But those which exist in rocks nearly horizontal, "must be referred exclusively to the removal of the substance that once filled them; and the cause of that removal appears to have been a violent and transient inundation." Valleys of this kind have been designated by the very inappropriate term, "Valleys of Denudation," as if they had been only *exposed*, not *formed*, by the catastrophe. Many circumstances seem to oppose the diluvian hypothesis, in reference to the origin of valleys; among which, the following may be noticed.

a. *Shape of Valleys.*—The valleys of denudation are not always straight in their course; they have their salient and re-entering angles, their lateral branches, and their increase in width as they descend. When we look at a valley, at present forming, by the action of running water, in beds of clay or gravel, we witness the s[illegible]its banks produced by the oscillations of the strea[illegible]ow transporting the materials from one side, the[illegible]nd thus aiding the force of gravity in causing the loose matter of the bank to descend. The lateral branches are produced by a similar process; and the valley widens as it advances, by the increase of its waters from the lateral streams, and the consequent increased transporting power. I am in the habit of employing an old-fashioned logic, and comparing small things with great, referring analogous phenomena to the same cause, and proceeding from the distinct to the obscure. Under the influence of these principles, I feel myself compelled to conclude, that the old valleys, with the characters described, have been produced, like those forming under my eye, *by the long-continued action of running water at the bottom.* How a sudden, transient and universal flood, covering the highest hills, could have produced these effects, I cannot conceive. The main branch must have been first scooped out; then the subordinate lateral branches, in succession; and a current in the main branch following each, to clear away the rubbish. Had the lateral currents been flowing simultaneously with the principal one, a *bar* would have been formed at the mouth of each branch; and if there had been no *succession* of currents in the main trunk, it would have been filled with the materials of the lateral branches. To those who have studied the natural history of rivers, especially their junctions with other rivers or with friths, the force of the objection will be obvious.

It has been objected to the theory of the excavation of valleys by running water, that *now* no water flows through them. But water *may* have flowed through them, though now absent. The bursting of a lake, at a higher level, may have cut off the sources of several springs, and directed water through a distinct and very different channel from that in which it formerly flowed.

b. *The Impotence of Water as an Abrading Power.*—The advocates of the diluvian hypothesis, have, in their zeal, committed that mistake intimated by the schoolmen, "Causam assignare quæ causa non est." It is impossible to form an adequate conception of all the effects which might result from a violent and transient inundation, covering the highest hills, and sweeping whole continents with destructive fury. The mind is lost in the vastness of the operation, and the imagination is left, unfettered, to pursue its reveries,—a most bewitching predicament for a geologist. But we may make an approach to the subject. When a river is in a violently flooded state, we witness it remove the soil which opposes its current, transport to a lower level the *loose blocks* of rock, and sweep away the animal and vegetable productions occurring in its course. But it is subject to certain limitations. Throughout its course, its velocity is greatest at the surface and the middle of the stream, from which it diminishes toward the bottom and the sides, where it is least. When it enters a hollow, lake or mill-pond, the water below the outlet has its motion checked, and, in its state of comparative stillness, permits the heavier materials it had transported to subside. When a water-spout descends almost in a solid column of great height, and exerting, consequently, a pressure well calculated to remove obstructions, it penetrates the soil, and disperses it, along with the vegetable covering, removes the loose blocks of stone, and the surrounding detritus, while it makes but a feeble impression on the solid rock. When an alpine lake bursts its barriers, it acts precisely as a river in a flooded state; carries along with it soil, loose rocks, trees and animals, depositing at a lower level the wrecks of its course,—as happened in the Val de Bagnes, by the bursting of the lake of Mauvoisin, (*Edin. Phil. Journ.* No. 1. p. 190.)

Let us now suppose a body of water (no matter at present whether fresh or salt), of sufficient height to cover the highest mountains, and possessing a progressive motion of great velocity, suddenly to arive at the north of Zetland, traverse the kingdom, and pass off towards the south, at the Land's End, What would be the accompanying phenomena? The soil would be every where annihilated in its progress, and, as mud, transported to a distance. The animal and vegetable inhabitants would be floated off. All detritus, boulders, and loose blocks of rocks, would, at the onset, yield to its pressure and velocity. But every lake, every valley, every lee side of a hill, every frith and bay of the sea, would speedily be in a state of comparative stillness, and receive the largest and the heaviest of the transported blocks. In the bottom of valleys and lakes we should now find the wreck of the catas [illegible], have we the shadow of evidence to warrant the [illegible] t this inundation could tear up solid rocks, and [illegible] ns in undisintegrated strata? No.

The force of cohesion, or rather crystallization, is more than a match for water falling from any conceivable height, or moving with any known velocity. The numerous islands which occur around our coasts, even where most exposed, and the cascades so common in the hilly districts, attest the absence of this abrading or excavating power. Did it possess this power, the Straits of Dover and the Pentland Frith must by this time have become unfathomable; Niagara should have ceased as one of the wonders of the world, and wooded valleys should have occupied the place of the Canadian lakes.

While I deny to water this *abrading* power, because the whole history of rivers is in opposition, I willingly admit its *transporting* power after disintegration has taken place,—a distinction to which the student in geology would do well to take heed.

c. *The Terraces in Valleys.*—In many valleys, on the Continent of Europe, in this country, and in America, terraces occur in the banks, which, from their horizontality, indicate their production by water at the period these valleys were lakes. Several terraces may be traced in some valleys, and these, according to Professor Buckland, "shew the number of successive stages by which the bursting of the gorge took place."—(*Rel. Dil.* 217.) In Lochaber four such terraces occur, shewing four successive eruptions. These terraces, however, are declared to be "all of postdiluvian origin."—(*Ib.*) Whatever be the era of these terraces, they demonstrate a few truths, which cannot be very agreeable to the supporters of the diluvian hypothesis. Many lakes formerly existed, where valleys now occur; and there are agents in Nature capable, at different intervals, of opening the barriers of these lakes, and permitting the water to escape suddenly. Such lakes and such agents may have existed before the flood. Each bursting must have resembled a deluge in its effect upon the district through which the waters passed, and the wrecks which it would accumulate at the lower level. When, therefore, we witness a valley, the present waters of which empty themselves by a narrow gorge, how are we to determine whether that gorge has been opened before the deluge, at the deluge, or after the deluge? The Vale of Pickering, in Yorkshire, may be taken as an example. According to Professor Buckland, it was an antediluvian lake (it would have been, from its characters, a valley of denudation, had it not been necessary to have a sheet of fresh water for the antediluvian hippopotami to swim in); the deluge opened the gorge at Malton, and converted it into a postdiluvian valley. But it is just as probable that it was a postdiluvian lake, and that the gorge of Malton was removed by an agent, similar to that which opened its northern neighbours in Lochaber. When we see a valley, the waters of which flow out at a gorge, we may infer that it was formerly a lake. We may also infer that a sudden deluge could not tear away the barrier rocks, unless previously disintegrated; and we may watch the transporting power of the present stream: but if we have any geological caution, we will hesitate about fixing the era of the change.

These terraces are found in greater numbers in alpine districts, as might have been anticipated. They occur, however, even at low levels. I have already noticed three examples in this Journal, and I have more to produce. They are much more numerous than is commonly imagined. Even in the valley of the Thames there is reason to believe they exist, though this hollow is pronounced, by Professor Buckland, a valley of denudation *.

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* In the "Reply" I am accused of supporting one of my conclusions "by stating, on the *misinterpreted* authority of Mr Trimmer's paper," that several of the reputed antediluvian animals occur in the postdiluvian, regularly stratified clay, &c. But how is this grave change of misinterpretation supported? "I venture (he says) to assert, that no remains of this kind have ever been found in the peat bogs of any part of the valley of the Thames, still less in the regular stratified clay, *that is, the London clay*." Had I really said that Mr Trimmer found these remains in the "London Clay," the charge would have been well founded, as he says that they occur *above* the *London clay*. But I say no such thing, Is the London clay (in the geological sense of the term) the only *regular stratified clay* with which my opponent is acquainted? This cannot be the case. Or can he deny, that the "Brentford clay" is less regularly stratified than the "London clay?" I use the phrase, *obviously* consistent with the authority which I quote; and I was the more inclined to do so, for the purpose of exhibiting the distinction between this *regularly stratified* clay and the ordinary diluvium, which is *irregular* in its stucture. So far, therefore, *I* have been misinterpreted, not Mr Trimmer. But there is still a difference between us. Professor Buckland says, that he has visited the clay in question, and pronounces it [redacted] Last spring, when in London, I was anxious to see a genuine exa and the more so, as Mr Trimmer's remarks indicated a very n: and because I had suspected that the advocates of the were in the habit of confounding together, at

Mr Greenough, a strenuous supporter of the diluvian hypothesis, has stated in his Geology (p. 121.), that "the valley of the Thames, in London, is contained in that of which Clapham Rise forms part of the boundary on one side, and the Green Park on the other; and this, again, is contained in the larger valley, which occupies the interval between Highgate and Sydenham. Arrived at these points, we find our horizon bounded by a chalk ridge still loftier." These included valleys throw great light on the history of the globe. They are like the circular valleys in river courses: they mark some of the features of a former state of things; they assist us in tracing the changes which have taken place, and even the agents concerned in their production: but they give us no dates.

11. *Formation of Gravel Beds.*—The materials of which these beds consist, appear, in general, to be rounded blocks of rocks, confusedly mixed together, or presenting but indistinct marks of stratification. The blocks are seldom angular, and never exhibit the surfaces or edges of a mass recently detached from an undisintegrated rock. As these masses are supposed to have been derived from the rocks which the geological deluge tore from their beds during the excavation of the valleys, we might expect to find them exhibiting numerous instances of tolerably fresh-

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least, two of the "modern strata." Nor was I disappointed; for that which my opponent has pronounced diluvium, I found to be *Lacustrine Silt*; and my conclusion rested on the following facts: 1. The beds, and their strata of fine clay and sand, are nearly horizontal. 2. They contain, here and there, thin horizontal patches of small rounded flinty pebbles, (precisely similar to small layers of gravel which I had seen in genuine examples of similar origin), indicating the influence of occasional floods. 3. Scattered through the clay, I observed several pieces of shells, the present inhabitants of our lakes or slow running streams, viz. *Helix peregra* and *complanata*, *Turbo fontinalis*, and *Cardium corneum* of Montagu. It is evident, therefore, that a lake existed here which has been filled up by slow degrees, and the character of the materials, and organic remains of the different beds, mark certain epochs in the process. It is fortunate that this example occurs so near London as to be of easy access to the members of the Geological Society. Perhaps a good deal of the reputed English diluvium may, upon investigation, be found to be lacustrine silt, as in the present instance.

fractured surface, and the edges and corners still nearly entire. But when we find the reverse of all this generally to be the case, we must draw the conclusion, that the fury of the agent, which collected the contents of these beds, was chiefly expended on the loose and weathered blocks on the surface. This is a fact of some value, especially when viewed in connection with other characters exhibited by the gravel.

The clay or loam associated with the gravel, according to Professor Buckland, " possesses no character by which it is easy to ascertain the source from which it has been derived, *but usually varies with the nature of the hills composing the adjacent districts.*"—(*Rel. Dil.* 191.) On the supposition that this loam was derived from the finer portions of the soil and detritus removed by the waters of the deluge, we might expect that it would possess something like a common character, not in England only, but over the globe. But when we see it vary with the nature of the neighbouring hills, and consequently with the soil and detritus which they produce, we are irresistibly led to infer the operation, not of a universal, but of a *local* agent.

According to Professor Buckland, the " diluvial gravel is almost always of a compound character, containing amongst *the detritus of each immediate neighbourhood, which usually forms its greatest bulk,* rolled fragments of rocks, whose native bed occurs only at great distances, and which must have been drifted thence at the time of the formation of the gravel, in which they are at present lodged."—(*Ib.*) The *rolled* character of the gravel is fatal to the supposition of a *sudden* and *transient* inundation, acting upon fresh portions of dislocated strata. The circumstance of some of the blocks having travelled from a distance, is equally satisfactorily explained, on the supposition of a partial flood, occasioned by the bursting of an alpine lake, as by a sudden and universal flood. We can scarcely, however, avoid asking the question, Would not a general flood, raging violently, have produced gravel, of so confused and mixed a character, as to render it difficult to trace the origin of its materials? This *local* character though apparently hostile to the diluvian hypothesis, is [illegible] to society in an economical point of view. Norway [illegible]ch from this transient flood, for, according to Pr[illegible], pebbles of her rocks have been carried to England. But our country has been more highly favoured. Had it been otherwise, instead of gold reposing at the base of the Leadhills, or stream-tin in Cornwall, they had been resting far from their birth-place; probably, if the deluge was from the north, in the bottom of the Bay of Biscay.

"There is one character exhibited by the boulders in the gravel, of a truly interesting kind, in a theoretical point of view,—the intervention of valleys between the rocks from whence they came and the station they now occupy. It seems to be admitted on all hands, that these valleys did not exist at the period of the transportation of the gravel. Mr Greenough declares, that "the blocks of granite on the Jura attest the non-existence of the Lake of Geneva at the time of their transportation,"—(*Geol.* 177.); and, according to Professor Buckland, "the quartzose pebbles found on the tops of the hills round Oxford and Henly, were drifted thither from the central parts of England, before the excavation of the present valley of the Thames."—(*Rel. Dil.* 248.) If, then, we consider the gravel as diluvian, the valleys must be regarded as *postdiluvian*; or, if we consider the valleys as having been formed at the deluge, then the beds of gravel must be regarded as *antediluvian*. Professor Buckland has endeavoured to avoid the admission of these conclusions. "It seems probable that the *first rush* of these waters drifted in the pebbles within the great escarpment of the oolite, and strewed them over the then nearly continuous plains; and that the valleys were *subsequently* scooped and furrowed out by the *retiring action* of these same waters."—(*Rel. Dil.* 253.) Is it conceivable that this sudden, transient and impetuous deluge, should have transported, in its first rush, various kinds of boulders, ten, twenty, or hundreds of miles, strewed them over nearly continuous plains, and then proceeded to scoop and furrow out numerous, deep and extensive valleys in these plains, whilst it permitted the deposits of its first rush to retain undisturbed possession of the station to which they were first brought? Could I bring my mind to assent to such statements, I should claim to rank with Judæus Apella. But the difficulty does not end here. In these valleys, supposed to have been excavated by the *retiring* waters, extensive depositions of gravel occur. (*Rel. Dil.* p. 251–2.) This last circumstance, which is far from uncommon, marks a third epoch in the history of valleys and gravel. In the first period, the gravel was transported across continuous plains. In the second, valleys were scooped out. In the third, the bottom of these valleys received deposits of gravels. These facts intimate successive operations, executed under different circumstances, and seem fitted for leading to the inference, that some time intervened between the several changes. They certainly do not support the conclusion, that the three phenomena had their origin in the same sudden and transient inundation. Under all the circumstances of the case, the young geologist will feel himself without a guide, and without a test, in determining the æra of the formation of a bed of gravel. 1. It may be antediluvian, produced by the bursting of a lake (for lakes must have been numerous, indeed, and extensive, before the excavation of so many gorges and valleys by diluvial action), spreading its wreck on nearly continuous plains. 2. It may be the result of the first rush of the diluvian waters, previous to the formation of the valleys of denudation. 3. It may be the wreck of these valleys, produced during the tumult of the retiring waters. 4. It may be the result of the very last effort of the flood, to fill up the frightful excavations it had produced in the fury of its retreat. 5. It may be postdiluvian, and the result of the bursting of an alpine lake: and this gravel may have been deposited at very distant intervals. On the banks of Glenmornaalbin, diluvium may occur, referable to four different burstings of the Lochaber lakes, and all of them prior to human record. The diluvium of Martigny, from the bursting of a lake, was formed in 1818. When all these probabilities are taken into consideration, few, who generalize with ordinary caution, will feel inclined to refer to one æra the formation of all our irregular beds of clay and gravel.

Independent of the depositions of confused portions of gravel and loam, there are likewise extensive depositions of sand, and gravel, and clay, of the same materials as the so-called diluvium; but which, by being divided into beds and strata, indicate a subsidence from water in a state of comparative stillness. The chara [illegible] beds seem to have been in a great measure overl [illegible] advocates of the diluvian hypothesis. It is not p [illegible] h beds could have been produced by a sudden and transient flood, which, in its first rush, transported "Norwegian pebbles" to the plains of England; and, by the impetuosity of its retiring waters, scooped out the Solway Frith, the English Channel, and the Lake of Geneva. On the other hand, a lake at a high level, bursting its barrier, and carrying the wreck into a lake at a lower level, would give origin to stratified gravel, sand and clay, such, for example, as may be seen in the neighbourhood of Edinburgh, and on the south bank of the estuary of the Tay; and which lower lakes have in their turn been drained.

The last character which I shall notice belonging to those beds of loam and gravel supposed to have been formed by the deluge, is the presence, exclusively, of the remains of *land animals*. This fact is supported by the testimony of Professor Buckland, in his "Inaugural Lecture," and "Reliquiæ Diluvianæ;" by Mr Greenough in his "Geology;" and by Mr Conybeare in the "Geology of England and Wales." This character yields a demonstration, that the water, which in its fury produced or transported this gravel, passed over a portion of the Earth's surface, on which dwelt land animals, and that a flood from the sea had not been concerned in the phenomena in question. To the matter confusedly brought together by this flood or floods of *fresh water*, I have, in my second paper on the "Modern Strata," given the name of *Lacustrine Diluvium*. Had a sudden, universal and transient deluge been the agent concerned in its formation, then should we have looked for the remains of the *animals of the sea*, mingled in sad disorder with those of the land and the lakes; or rather fishes, shells and zoophytes, where we now find the wreck of land animals [*]. Even the peculiarities of the remains of the land animals stand opposed to the geological deluge as it has been interpreted; for these belonged to individuals, which, according to Professor Buckland, " lived and died in the regions where their remains are now found, and were

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[*] In my first paper I had enumerated *five* characters of lacustrine diluvium, indicating, that a universal flood had no share in its formation. Four of these characters are admitted, directly or indirectly, in the "Reply." But the fifth ("the absence of marine exuviæ,") is brought forward against me as an example of "*misstated facts*:" and it is added, that if I had ever seen or heard of threa examples, which are quoted, of the presence of marine remains, I never would have advanced such an argument. One of these examples is unfortunate, as the learned Professor seems to confound three different formations,—the crag, or *upper marine formation*; distinguished from those of the *modern epoch*, by the species of shells, but especially the zoophytes, which it contains:—the *Lacustrine Diluvium*, containing the remains of land animals:—and *Marine Diluvium*, containing the relics of existing marine shells of the neighbour-

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ing sea. In my second paper, I intimated my acquaintance with his two first examples, and I added six others, with which he might have been acquainted. Yet my opinion remains unchanged; and I misstate no facts, while I preserve a distinction in geology (which my opponent will soon find it necessary to adopt) between Lacustrine and Marine diluvium. In the appendix to his paper he recurs to the same subject, and considers, that the facts I advance in my second, are in " direct contradiction " to the opinions advanced in the first. Here he labours under *ignoratio elenchi*, which a reperusal of my two papers would readily remove. If I allow him to use *my terms* with *his different signification*, I have too much respect for his logical powers to anticipate a failure in his object. But if the terms I use be taken in the sense in which I have defined them, the charge of " contradiction " will be found without proof.

Professor Buckland, rising, as it were, in his demands, having fancied that I had contradicted myself, announces the cause of my misfortune and the extent of my guilt.—" *Not being aware* of facts which so materially affect his argument, at the time of his writing the paper in question; at any rate, it would have more *candid to acknowledge his error*, than to leave to me the task of pointing it out, and applying it to my advantage in the matter at issue between us." Is it *probable* that I could have been ignorant of *eight* facts at the time of writing my first paper, which I give in detail in the *continuation*, or second paper; or that I would record these eight facts in the second paper, which contradicted my statements in the first, without offering any explanation? Low, indeed, must be my rank in the intellectual scale, in the opinion of my opponent, if he be disposed to reply in the affirmative. But I can produce evidence that it was not *possible* I could be ignorant of *some* of the facts at least, stated in my second at the time I wrote the first paper, nor for eighteen years previous. The first of the eight examples of marine diluvium in Scotland which I quote, is from a *published paper of my own*, and to which there is a particular reference, on a bed of sea-shells, on the south banks of the estuary of the Forth. This bed, *as stated in my second paper*, I examined in 1806, read an account of it to the Wernerian Society in 1811, and published this account in the Annals of Philosophy for August 1814. I may even go farther, and say, that it is *not probable* that Professor Buckland was ignorant of this *demonstration* of my previous acquaintance with these reputed contradictory facts. He quotes Captain Laskey's paper on the marine shells of the Paisley Canal, from the Annals of Philosophy for February 1814, and my paper referred to appeared in the some work, in the number for August of the same year. [redacted] Memoirs, which he also quotes, contain a similar reference. I vincing proof of all (on the supposition that he read the pap riticise) is the fact of this example of marine diluvium bei icts I adduce in illustration of the history of not drifted thither by the diluvian waters from other latitudes. (*Rel. Dil.* 44.) It is impossible for me to form a conception of a sudden, violent, transient, and universal flood, which transported Norwegian pebbles to England, yet did not bring along with these a few carcases of the truly arctic animals, such as the white bear; neither floated off to Africa the land animals which were browsing on the continuous antediluvian plains of England. To me it is equally inconceivable, that the inhabitants of southern and tropical countries, were not drifted northwards, and a few of them left in England by the agency of the retiring waters. Yet our diluvium contains not the productions of the polar or equatorial regions, but exclusively the remains of the early inhabitants of the British soil. This character furnishes another demonstration, that the agent or agents concerned in producing the diluvium, must be regarded as having possessed only a limited or local authority. We must be careful here, not to confound with "Lacustrine Diluvium," deposits on which I have bestowed the title of "Marine Diluvium." Portions of this diluvium have been formed within the period of authentic history; other portions are of earlier origin. The bones of land animals may occasionally be expected to occur in this formation, as the inundations of the sea, by which it has been produced, might have mixed with the spoils of the deep the relics of the dead or living terrestrial inhabitants which it met with in its progress.

3. *Mud in Caves.*—In the celebrated cave at Kirkdale, there is a layer of mud in the bottom, inclosing the fossil bones, and over this bed there is a covering of calcareous stalagmite. Professor Buckland considers the bones to have been carried in by hyænas as their food, when they dwelt in this den anterior to the deluge; that the mud was introduced by the waters of the deluge; and that the stalagmite is decidedly postdiluvian. (*Rel. Dil.* 48.). Another explanation is offered by the same author, of the mud and bones which occur, nearly filling several

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the formation. By quoting in the "Reply" only the *last* of my *eight* examples, the reader may be misled into the belief that the reproach is merited. Whereas, had the *first* of them been quoted, as justice required, it would have carried on its front the refutation of the charge of ignorance and want of candour it has been somewhat hastily brought forward to support.

caves in limestone rocks at Plymouth. Instead of having recourse to hyænas as carriers of the bones, he says, "that the animals had fallen during the antediluvian period into the open fissures, and there perishing, had remained undisturbed in the spot on which they died, till drifted forwards by the diluvian waters to their present place, in the lowest vaultings with which these fissures had communication." *Rel. Dil.* 78.

The safest way of proceeding, in such circumstances, is to endeavour to discover some analogous phenomena, the history of which is not involved in obscurity, and apply the explanation which offers itself in the last cases to the illustration of those which are more ancient and obscure. Fortunately such cases are accessible. In Wokey Hole, in the Mendip Hills, a cave occurs with lateral chambers; mud likewise occurs; and in this mud are found human bones, and a piece of a sepulchral urn. These *bones* are said to be "very old, but not antediluvian." Where is the proof? or how are we to distinguish between antediluvian and postdiluvian bones? The mud, too, is "evidently fluviatile, and not diluvian. How are we to distinguish between fluviatile and diluvian mud? Not by their contents, for bones are present in both. Not by a difference in juxtaposition, for both occur in caves with the floor as their bed, and stalagmite as a covering. The evidence, however, of the mud being fluviatile, may be considered as complete, as the spot on which it rests is within reach of the highest floods of the adjacent river. It may thus be assumed as a fact, that local inundations or floods are capable of conveying mud into caverns, and depositing it on their floors, under circumstances perfectly analogous to the so-called "diluvian mud," and of surrounding "postdiluvian bones" as the diluvian mud is supposed to have surrounded "antediluvian bones." In another cave in the same neighbourhood, numerous bones and skulls of foxes were found. It is likewise stated by Professor Buckland, that, at a little distance from the Cliff of Paveland, "is an open cavern, to which it is possible to descend only by a ladder, and which, like the open fissure at Duncombe P [redacted] tains at its bottom, and in the course of its descent, the etons of sheep, dogs, foxes, and other modern anim ally fall into it and perish." In reference to th ls and accumulations of bones, the learned professor offers the following sensible observations: "Animals at this day do fall continually into the few fissures that are still open; and carnivorous, as well as graminivorous animals, lie in nearly entire skeletons in the open fissure at Duncombe Park, each in the spot on which it actually perished, upon the different ledges and landing places that occur in the course of its descent; and from which, if a second deluge were admitted to this fissure, it could only drift them downwards, and with them the loose angular fragments amidst which they now lie, to the lowest chambers in which the bottom of this fissure terminates." (*Ib.* 78.) The *bones* in caves may have been drifted in from open fissures at a high level by water, whether in the character of a local or extended inundation; and the *mud* may be referred to a similar origin. But, in all this, there seems no ground to infer the exclusive agency of one sudden and transient deluge, when causes still exist, though of a more humble kind, adequate to produce the phenomena.

The cave of Kirkdale does not present any appearances, warranting an explanation different from that which applies to acknowledged postdiluvian fissures and caves. The rounded cavities in the bottom of the cave, resembling, according to Mr Young, "such water-worn hollows as we see in rocks, in the beds of rivers, or on the shores of the ocean," prove, that, at a period antecedent to the introduction of the bones, this was a fissure in the limestone traversed by a subterraneous river. This is rendered more than probable, by the numerous other fissures existing in the same bed, into one of which, in the immediate neighbourhood, the Rical Beck enters, and for a certain space becomes a subterranean river *. We have here, therefore, an agent capable of bringing in the mud and bones from higher fissures, if such existed, and depositing both in their present situation. The existence of such fissures cannot be doubted, since Professor Buckland has made the concession. "The fact already mentioned of the ingulfment of the Rical Beck, and other adjacent rivers, as they cross the limestone, showing it to abound with many similar cavities to those at Kirkdale, renders it likely that other deposits of bones may hereafter be discovered in the same neighbourhood." But are there no open fissures in this bed of limestone still existing, as natural pitfalls for modern animals, and furnishing intimations of the former state of the district? "In Duncombe Park, in the immediate neighbourhood, and in the same limestone rock, there is at present an irregular crack or fissure twenty feet long, and three or four feet broad, which is almost concealed and overgrown with bushes, and which being nearly at right angles to the edge of the cliff, lies like a pitfall across the path of animals that pass that way. It descends obliquely downwards, and presents several ledges or landing places, and irregular lateral chambers, the floors of which are strewed over with angular fragments of limestone, fallen from the sides and roof, and with dislocated skeletons of animals that have, from time to time, fallen in from above and perished." (*Rel. Dil.* 55.) The fissure was found to "contain the skeletons of dogs, sheep, deer, goats and hogs." "The bones lay loose and naked." A local inundation flowing into the fissure would transport the bones to the lowest chambers, and leave them in the same circumstances as the so-called antediluvian bones. The evidence thus appears to be in favour of that opinion, which supposes that the bones in the Kirkdale cave were brought to their present situation from caverns at a high level, by the agency of water, which deposited at the same time the mud in which they are imbedded. I say imbedded, because the mud does not appear simply to have filled up the interstices or layers of bones, but to have *suspended* and enveloped many of them. "Most of them are broken into small angular fragments and chips, the greater

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* The proof which is brought forward by Professor Buckland, that the Kirkdale Cave was not formed or modified by the agency of water is singularly defective. The sides "are constantly *rough*." Were they never smooth? The limestone in which fossil shells are imbedded decays more rapidly than the relics it encloses, when exposed to the weather or to damp air; as the surface of every secondary limestone testifies. (Take the columns of St Paul's as an example.) Nor is the proof, that the bones in the same cave could not be introduced by running water, more satisfactory; "because it is impossible that now, or at any past period of time, any river should ever have flowed there." A river flows, at this moment, not a hundred feet distant, and its channel is only 36 feet lower than the cave. There are many other rivers in

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the neighbourho [illegible] er the same bed of limestone, in which the cave is situate, [illegible] of fissures. The reader, from these facts, will be able to e [illegible] a *geological impossibility.*

part of which lay separately in the mud."—(*Rel. Dil.* 12.) The present existence of pitfalls, and subterranean rivers in the same limestone, likewise gives strong probability to the inference which we have drawn, or rather would amount to a proof, provided there be nothing in the condition of the bones themselves, justifying the propriety of another explanation.

The bones in the cave are chiefly fragments, and besides the small splinters, numerous portions of the ends, or the most solid portions of the larger bones, the jaw and teeth, occur. Some of these splinters are angular, "but many others were decidedly rounded and smoothed at the projecting parts, bearing obvious marks of having been long agitated by water."—Young, *Wern. Mem.* iv. 266.) These circumstances confirm the supposition, that the bones were drifted into their present position by water, especially when we keep in view, that the bones of the different species were found co-extensively distributed "even in the inmost and smallest recesses."—(*Rel. Dil.* 16.) Professor Buckland, in endeavouring to establish his hypothesis, that hyænas dragged in the bones in question, considers the rounding of the fragments as having been produced by the treading of the animals in the bottom of their den. His chief argument, however, is derived from indentations which are observable on some of the bones, and which he refers to the nibbling of the hyænas while cracking the bones, in order to extract the marrow. Even admitting that these indentations have been produced by the teeth of hyænas, (an opinion not rendered even probable), still we would adhere to the explanation already given, since these markings may have been produced by hyænas on the bones as they lay in the original pitfall, to which these depredators may have had access. In reference to the marks or pits on the ulna of a wolf and the tibia of a horse, occasioned, in the opinion of the Professor, by the canine teeth of an animal of the size of a weasel, he adds, "These pits must have been formed before the bone was imbedded in mud in the lowest recesses of the cave, and probably whilst it lay exposed in some upper cavity of the rock." Why refuse to adopt a similar explanation of the larger markings on the Kirkdale bones? But, if the hyænas carried in *all* the bones, it may be asked, why did they transport those of such small animals, as water-rats, weasels, rabbits, pigeons, snipes, and even larks,—animals, which to a hungry hyæna, would not be a mouthful. But the difficulty increases when we consider, that, if the evidence is conclusive to prove that the hyæna carried in the bones of the elephant and rhinoceros, and reduced them to fragments, it equally proves that the small bones of these animals were carried in by the same agent; nay, more, that the hyæna which gnawed the bones of an elephant, condescended to pick the flesh from a mouse, and separately break its jaws and legs. This would prove too much.

The circumstance of Professor Buckland discovering some rounded pieces or balls, which he considers as the album græcum or fecal matter of the former inmates of the den, at first sight strengthens his conjecture. Mr Young says, that, "having observed some pieces of bones nearly in the same state, I am not without suspicion that the whole may be portions of bone, decomposed in the cavern, and reduced to their present form by a mixture of water and other ingredients." Without venturing to give an opinion respecting this disputed matter, I may add, that, even viewing it as the fecal matter of hyænas, it too could be carried in by a flood as easily as the os calcis of a water-rat, the jaw of a mouse, the ulna of a lark, or the shoulder-blade of a small duck. The evidence proving the Kirkdale cave to have been an antediluvian den, thus seems, in all its parts, so deficient in precision, as to warrant the rejection of that hypothesis it has been produced to support.

In several caves (some in such circumstances occur in the neighbourhood of Kirkdale) the mud does not contain any organic remains. In such cases, the flood must have been truly local, or passed through caverns destitute of the skeletons of wild beasts.

Though the mud in some caves is continuous, in other cases it is distinctly stratified, intimating its introduction to the cave at different intervals. "In one large vault at Oreston, where the quantity of diluvium is very great, it is stratified, or rather sorted and divided into laminæ of sand, earth, and clay, varying in fineness, b [redacted] ferable to the diluvial washings of the adjacent countr artially interspersed with small fragments of clay ."—(*Rel. Dil.* 70.)

The last ci h I shall notice connected with the mud in caves, is the absence of similarity of colour and composition in different districts. In the mud of the geological deluge, produced from the wreck of Norway and England, or rather of the whole surface of the earth, we might expect the exhibition of a common character in all caves. But when different caves have mud of a particular local character, the inference is obvious, that the causes concerned in its production have likewise been *local.*

IV. *Extinct Animals.*—If ever a sudden, universal, and impetuous flood, sweeped our island in its fury, land animals must have been drowned and carried off, or, as Professor Buckland expresses himself, "every thing that lay without, on the antediluvian surface, must have been swept far away, and scattered by the violence of the diluvian waters."—(*Rel. Dil.* 39.) If we admit the truth of this statement, we should not expect in our country a single skeleton of a native animal, in our gravel, or loam, or in caves. Yet it is admitted that numerous relics of land animals, which lived and died in the country, are generally distributed in gravel, loam and caves. I am inclined at once to conclude, from these premises, that no such geological deluge ever occurred. Nor is other evidence wanting to justify the same conclusion. If these remains "were drifted from other countries to those in which we find them," we may ask, from what countries? Not from tropical regions, for the *species* of hyæna, elephant, and rhinoceros, the remains of which occur in our superficial strata, never were tropical animals, although from *name* the general reader may be betrayed to such an opinion. If these remains "floated backwards and forwards by the flux and reflux of the mighty currents then in motion, before the carcases became putrid, and the bones fell piecemeal into the gravel, as the agitation subsided," then should we expect to find the relics of the animals of arctic, temperate, and tropical regions, mingled in the same gravel; in other words, all the laws which regulate the physical distribution of animals would have been violated, and our gravel-beds would have been full of the monuments of the rebellion. Yet there is no such confusion; consequently there have been no such mighty currents.

Perhaps the most interesting fact in the history of the relics in our modern strata, is the occurrence in the same gravel of the bones of animals which have become extinct, with such as have been extirpated by the chase, and with such as still inhabit the country. This fact, while it throws great light on the early state of the animal kingdom, may be regarded as the death-blow of the diluvian hypothesis. The extinct animals were, according to Baron Cuvier and Professor Buckland, antediluvian, and perished from off the earth, by the destructive agency of the diluvian waters. The objection to this explanation is unanswerable. The diluvian waters must have drowned *all land animals;* yet many which lived in the reputed antediluvian world, still live and flourish, in the same countries where the remains of their progenitors lie interred. I can find no attempt to explain these facts, except that, in the *Reliquiæ Diluvianæ*, (p. 41.), there is mention made of certain species having "*re-established* themselves in the northern portions of the world since the deluge;" and by the same author (*Edin. Phil. Journ.* No. xxiv. 308.), of others "that have *repeopled* this country since the formation of the diluvium." The history of this re-establishment or repeopling not being given, we cannot examine the value of the evidence adduced in its support. But we may ask, if the geological deluge ever took place, from whence did the modern animals proceed which *repeopled* the country? If there was any place within the limit of the geographical distribution of our present animals which the diluvian waters did not reach, then it may be supposed, that, independent of the *sudden* and transient nature of the inundation, a place of refuge might have been found, to which these animals retired during the fury of the agitated waters, and from whence they might issue forth to repeople the desolated regions. But, the history of the geological deluge does not warrant such a supposition; nor, even if it did, would the difficulty be removed. We could not avoid drawing the inference, that the place of refuge for the deer and the ox during the catastrophe, might have yielded protection to the gigantic elk and the mammoth. If any great inundation occasioned the extinction of these reputed antediluvian quadrupeds, its ravages must have extended to the other species having the same distribution, feeding in the same meadow, or browsing in the same forest.

Perhaps the abettors of the diluvian hypothesis may have recourse to the Ark as the place where the modern species found a temporary asylum. Still we have to ask the proof of the establishment of that *law of exclusion*, under the operation of which the mammoth and his unfortunate companions suffered extinction? If these were not excluded, we have still to ask, what has become of the *postdiluvian pairs and their families*, of these now extinct species, since they outlived the deluge, but have since disappeared?

Under the conviction that the diluvian hypothesis did not explain the extinction of our early quadrupeds, and that the subject, even in the hands of Baron Cuvier, had not received the elucidation of which it was susceptible, I endeavoured, in my "*Philosophy of Zoology*," to establish the laws which regulate the Physical Distribution of Animals, as a preparation for studying the "Revolutions" which had taken place in the animal kingdom.

I there intimated, in general terms (for I could not spare room for more), the effects which the persecutions of man must have produced on the distribution of many species. At the request of my valued friend Professor Jameson, I extended these observations, in the paper on the "*Distribution of British Animals*," which appeared in the 22d number of this Journal. Subsequent reflection on the subject has only served to confirm the views I have brought forward, and to convince me that we must refer the extinction of these early quadrupeds to the destructive influence of the chace.

It is admitted on all hands, that the relics of the extinct quadrupeds, of those which we know to have been extirpated by man, and of those which still dwell in the country, are coextensively distributed, and must all have lived at the same time in this and analogous countries. From these premises, I may safely draw the following conclusions:—1. That the cause of extinction was not a general physical one, as it did not extend successfully to the subsequently extirpated and recent species. 2. That the cause of extirpation has not extended successfully as yet to the existing species. From the evidence of our observation, and the testimony of history, confirmed by geological documents, I am warranted likewise in the following conclusions:—1. Man is at present carrying on extirpating operations against many species; nor is there room to doubt, that in any age he ever was otherwise occupied. 2. Different species vary in the extent of their resources to resist these extirpating efforts. 3. The *individuals* of many species have been greatly reduced in numbers by these efforts. 4. *All the individuals* of several species have been destroyed by these efforts, in this country, even within the last six or eight centuries. 5. If extirpation has taken place to such an extent, within the period of a few centuries, how manifold must have been its effects during the six thousand years that man has lorded over the creation. To such efforts do I ascribe the extinction of our ancient quadrupeds; and the inductive reasoning which led me to the opinion, carries along with it all the authority of demonstration.

To the explanation which has thus been proposed to account for the extinction of certain quadrupeds, several objections have been offered by Professor Buckland in his " Reply (No xxiv. 312.) They seem, however, to have originated in imperfect notions respecting the " Distribution of Animals;" and, therefore, readily admit of an answer.

" 1. Is it not incumbent on him first to show at what period such animals as these, much too formidable to be overlooked, were ever known to have existed?" I do not think the proof called for with propriety. The events referred to were not sufficiently striking to arrest the attention of the public; and there were no " Journals" in those days.

" 2. Can he give any reason why hyænas should have been extirpated at a more early period than wolves, had they ever existed in postdiluvian Britain?" Yes. Their resources against the efforts of the sportsman must have been fewer and less efficacious. The proof rests on analogy. The wolf has been extirpated, but the fox remains. The bear has been extirpated, while the badger remains. If we pass from Britain to the Continent, similar proofs occur. The gigantic elk has been annihilated, while the Scandinavian elk remains. If we pass from Europe to America, still there are proofs: the musk ox has perished in E[illegible] it exists in America.

" 3. Is it [illegible] e savage hordes which inhabited Germany befor[illegible] y the Romans, should have utterly destroyed such powerful animals as the elephants and rhinoceros, as well as the hyæna, from the impenetrable fastnesses of the great Hercynian forest, when animals of the same kind have not yet ceased to abound in the woods of India, and the wilds of Africa, in spite of a farther persecution of nearly two thousand years?" Quite probable. The objection is specious, not solid. Savages are good huntsmen; and those which inhabited the west of Europe were not destitute of energy, as the Romans found to their cost. Those of temperate and cold climates, must follow the chase eagerly, Ceres to them being niggardly. They, too, can commit their depredations with greater effect, aided by the seasons, and the migrations consequent on the changes thereof. But independent of these explanations, I too may ask, How have the wolf, and the bear, and the beaver been extirpated from Britain, while, in the neighbouring continent, " after a farther persecution," they still maintain their ground. The same explanation must apply to both cases,—the different facilities of the sportsman to gain his object.

" 4. Surely the theory of their extinction by the savage natives, preceding the Roman invasion of these countries, is a matter of the highest improbability; their existence at that time, and subsequent extirpation, is, in the utter silence of Cæsar and Tacitus, and all later historians, and even of tradition, a moral impossibility." I deny that the natives were savages at the period of the Roman invasion; and let the appeal be made to the writings of Cæsar and Tacitus. The silence of the Roman historians as to the destruction of native animals is of little moment. The process of extirpation is gradual, and had commenced long before Romulus and Remus had a being, or the wolf that suckled them. The historians were otherwise occupied; Cæsar, in recording his own achievements, and Tacitus in lauding the deeds of Agricola, and fabricating speeches for Galgacus. As for tradition, the learned professor rejects the testimony of the Niebelungen, a poem of the 13th century, which seems to refer to these extinct animals, because it records, at the same time, some *superstitious notions* of the æra in which it was written. What will become of poor Samuel Johnson's Tour a few centuries hence, with its *second sight?*

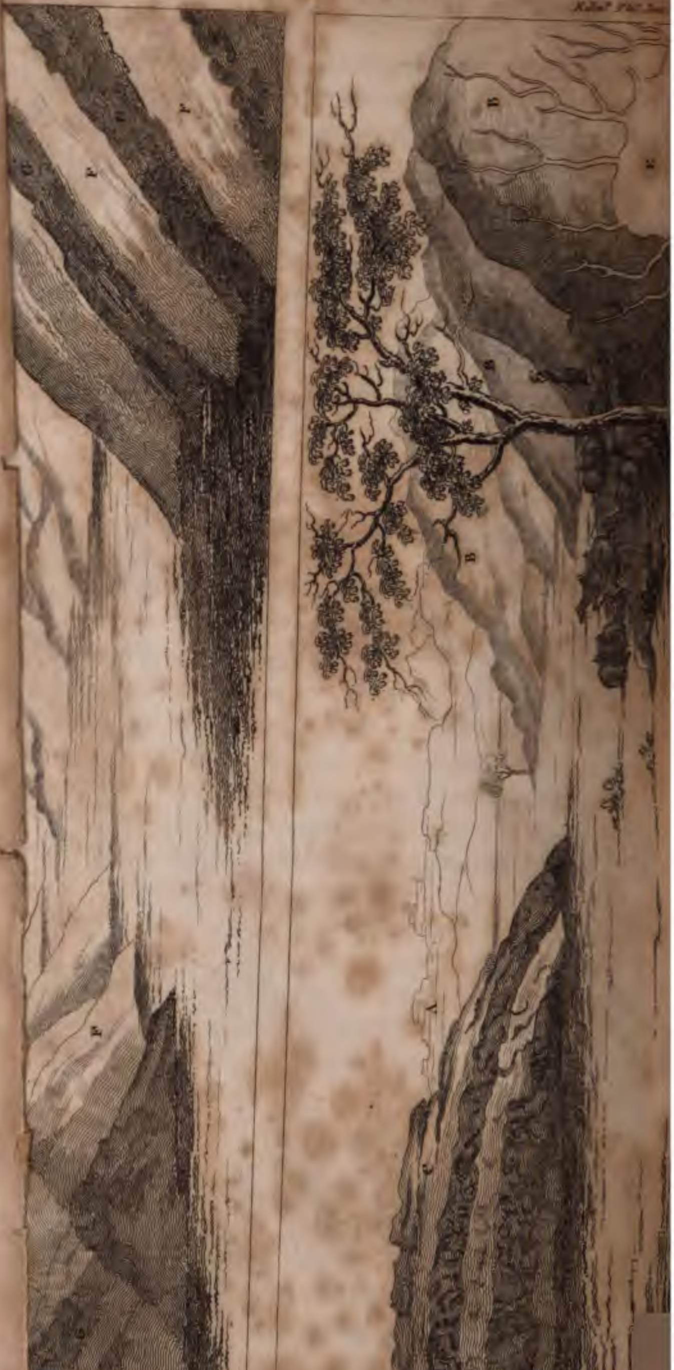
There is not in the whole range of this question, a single fact, in the history of animals, yet produced, which justifies, or renders probable the diluvian hypothesis. The whole science of zoology is opposed to it. Nor is phytology friendly to the cause.

If ever a mighty torrent of fresh or salt water committed those ravages on rocks and valleys, which it is represented to have done, the *soil* and *land-plants* must have been the first victims of its fury; and in our gravel, lakes and peat-bogs, we should now find the woods of tropical forests commingled with those which temperate regions produced, as they "floated backwards and forwards by the flux and reflux of the mighty currents then in motion," until they rested in the hollows of the surface, upon the retiring of the waters. The existence of land-plants, at present, on the surface, and the absence of the wreck referred to, attest the non-existence of this supposed catastrophe. Perhaps the plants have "re-established" themselves, and "repeopled" the desolated region? Where was the spot in which they enjoyed exemption from the fury of the diluvian waters? It must have been within the limits of their geographical distribution; and as each district must have had a separate sanctuary corresponding to the distribution of the *species*, the mighty torrent must have met with many checks in its progress. It may be added, that the animals when they returned to repeople the valleys of denudation, must have been scantily supplied with herbage; and centuries must have elapsed before the washed, waterworn rocks could furnish a support to the vegetable tribes.

Perhaps the advocates of the diluvian hypothesis, in the absence of all support from physical science, may give it as their opinion, that the Deity, immediately after the catastrophe, created new soil, re-created the plants, and re-created *a part* of the species of animals which had been destroyed. Is not the silence of Moses fatal to the conjecture? Would he have failed to record in the sacred volume this second magnificent display of creative power? Perhaps, in this case, there is much need to be reminded of the caution of the poet:—"Nec Deus intersit nisi dignus vindice nodus."

From th [illegible] ements, I feel myself warranted to conclude, That [illegible] of the *geological deluge*, in its effects, such as the [illegible] diluvian hypothesis describe, is, like

PLATE VIII.



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similar well meant inventions of their predecessors, Burnet, Woodward, and Whiston, disproved by the truths of Geology, the truths of Zoology, the truths of Phytology; and contradicted by the authority of Revelation.

Flisk, *24th December* 1825.

Art. II.—*Notice of the Rocks composing the Mountains which occur in the Desert between the Nile and the Red Sea.* With a Sketch. (Plate VIII.)

The sketch of the Desert between the Nile and the Red Sea (Plate VIII.), is from the pencil of a gentleman on whose accuracy we place the utmost dependence. We give it in the hopes of its proving a useful guide to any future geologist who may happen to travel the same route. The journey across the Desert, from the Nile to Kosseir, was performed in three days, halting only for two or three hours at noon and at midnight. The mountains in the centre are granite, porphyry, &c. One part of the road lay along the junction of the sandstone and primitive mountains; which line it was easy to trace by the eye for many miles, as observation was nowhere impeded by vegetation or soil.

The sketch was taken between the Nile and the Red Sea, about 100 miles from Ghinneh. The distant mountains at A (Pl. VIII.), are composed of limestone, alternating with dark beds of trap, impregnated with a large proportion of sand, flints, agates, &c. disposed at an angle of 10° to 15°. The hills at B are composed of a blue schistose rock, of about 45° NE. with occasional masses of greenstone, and a red porphyry or sienite, and sometimes asbestus. The beds at C, are composed of coarse brown sandstone, and lie under the limestone, in a parallel position, at an angle of about 15° N. They are separated by the beds D, composed of disintegrated greenstone, with white calcareous veins, forming a reticulated net-work around the nodules. These beds, C and D appear always to intervene between the schist and limestone. At E, there is a very singular appearance: a mass of perfectly white quartz is seen protruding itself into the schist, with veins branching from it in all directions. At F, the limestones are separated by beds, G, of amygdaloid, and filled with nodules of flint, agates, &c.

The valleys are filled up with detritus, and are nearly level. In a climate so dry and so conservative, where it never rains, the rocks present a novel aspect to the eye of a European. In the Desert, however, there are evident traces of torrents, perhaps the effect of water-spouts. It is wonderful that mountains of their height should not attract a regular supply of humidity from the atmosphere. The wells in the centre of the tract may be about 100 feet deep, excavated in the schist, and are generally brackish and sulphureous.

ART. III.—*On certain Circumstances connected with the Condensation of Atmospheric Humidity on solid surfaces.* By HENRY HOME BLACKADDER, Esq., F. R. S. E. &c. Surgeon. With a Plate. Communicated by the Author. (Concluded from p. 91.)

4. SOLID bodies, which are at the same time the worst conductors of heat, and are possessed of a strong hygroscopic property, or an organization corresponding in effect thereto, are those which have their temperatures reduced most speedily, and to the greatest amount, when exposed on a clear evening after sunset. Solid bodies which have no hygroscopic property, and are the best conductors of heat, are those which have their temperatures the last, and the least reduced of all others.

Of the class of substances which have no hygroscopic property, those which have the least capacity for heat, and have the least conducting power, have their temperatures the soonest, and most considerably reduced.

These positions might be shewn to be correct, by comparatively recent experiments and observations, but this has been judged unnecessary, as they are fairly deducible from facts already well known to every one, at all conversant with the subject. If then, wit [redacted] sitions in view, it can be shewn, that, on an evening pr v, polished metals may have moisture condensed es, without radiation, or any thing equivalent uisite to bring about that effect it may at least go some length in inducing the advocates of that theory to reconsider the grounds upon which its exclusive influence is supposed to be established. In aiming to do so, at least an attempt at brevity is, on the present occasion, indispensably requisite.

Though, in general, polished metals, when exposed after sunset, are, *cæteris paribus*, the last and the least dewed of all other bodies, they may acquire moisture in three several ways: 1*st*, Acting mechanically in preventing aqueous vapour from being dispersed in the air, at a time when the latter is not saturated with moisture, and when both the air and the metal are of the same temperature. 2*d*, Acting mechanically, in merely receiving or intercepting particles of condensed vapour in their descent, after the air has become super-saturated with moisture, and at a time when the temperatures of the metal, and of the contiguous air, are equal. 3*d*, Not acting as a simply mechanical agent, but as a cold body attracting moisture from damp air, of a somewhat higher temperature.

Of the *first*, we have various familiar examples,—thus, if, when the weather is both warm and dry, we approach the finger to a highly polished metal of the same temperature with the air, aqueous vapour is instantly observed to be condensed on the metallic surface,—or, if we breathe opposite to, and at some interval from a metallic or glass mirror, the polished surface is instantly more or less obscured, though the mirror be of the same temperature with the air, and the latter far from a state of saturation. The breath is completely saturated with moisture, and warmer than the air; but, though we expire with the utmost force of the respiratory organs, against the ambient air, which has the same temperature as the mirror, we shall not be able to discover the slightest obscuration, in the form of a haze or fog; for this only takes place when the temperature of the air has been reduced from 50° to 60° below that of the human body. Hence, the mirror acts mechanically in preventing the diffusion of the aqueous vapour. Pieces of unpolished metals, and other rough, solid, and non-absorbent substances, produce the same effect; though, from the optical property of their surfaces, the effect is less, if at all discernible. The same effect is also produced, by bodies possessed of a hygroscopic property; but as, at least, part of the moisture is quickly taken into their substance, its presence on their surface is still less to be detected, than on the rough surfaces of the non-absorbents. Lastly, When a polished metal, of the same temperature with the air, is placed over a vessel containing water, of a somewhat higher temperature, vapour is condensed on its under surface; and the same thing happens when it is placed on, or a little above, the surface of an open field after sunset. The vapour issuing from the ground is condensed on the side of the metal, which is directed to the earth, provided its superficies be of a certain extent,—for if very small, the mechanical effect becomes neutralized.

In this country, examples of the *second* mode in which metals become dewed, are less familiar than in such countries as Holland and the Netherlands generally. There, during the warm season, the cold produced by evaporation (as it is conceived), is seldom or never great, the air being usually so very damp that but a small reduction of temperature is requisite to bring it to a state of supersaturation.

Musschenbroek had remarked, that a low haze or fog was a concomitant of dew in Holland; and Dr Wells seems unnecessarily to have objected to this observation of the Dutch philosopher. I never saw dew forming on the grass in the Low Countries, without a haze being at the same time more or less apparent; and, in our own country, if the eye be directed, on such occasions, to the distant surface, it will be found that there is commonly a certain haziness of the lower air, though not so dense as to be perceptible within a considerable distance. "Respecting this point," Dr Wells observes, "I can aver, after much experience, that I never knew dew to be abundant except in serene weather;" and again, "I can assert, after much attention to this point, that the formation of the most abundant dew is consistent with a pellucid state of the atmosphere. Hasselquist makes a similar observation with regard to Egypt; ' [illegible] during the season remarkable for the most profuse de [illegible] hts,' he says, 'are as resplendent with stars in th [illegible] er, as the lightest and clearest winter nights i [illegible] From this it is pretty obvious, that his attention had been chiefly directed to the appearance of the heavens; and, it is not improbable, that the place where his observations were chiefly made, was not favourably situated for observing the state of the lower air, by directing the eye to the distant surface. But, even when there are no clouds, and when the stars may be considered both distinct and bright, we sometimes observe the moon to be surrounded by a hazy whiteness or circle; a sufficient though not the only proof, that the atmosphere may have no inconsiderable quantity of condensed vapour dispersed through it, at a time when it might be considered both serene and pellucid. Even in this country, however, opportunities are not wanting for observing all solid bodies indiscriminately dewed, even to the woolly and hairy coverings of animals. This occurs when the air contains much aqueous vapour,—when, during the night, there has been a copious deposition of dew, and towards morning the formation of a dense fog. An increase of this state of the atmosphere would give rise to what is termed a drizzling rain, or raw mist, called in the French language *bruine* *. On such occasions, the upper surface alone of a horizontal piece of metal is coated with condensed vapour; but, if it has been lying on the grass, both its sides may be moist.

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* According to Toland, who had no small acquaintance with the Northern languages and dialects, " Linguarum plus decem sciens ;" *dour* in Armoric, and *dúr* in Irish, are terms for *water*; and *daigr* in Armoric, and *deor* in Irish, import *drops* and also *tears*. Hence probably the origin of our term *dew*; and evidently that of *daig* or *daigy*, sometimes pronounced *daghy*, a word still in use in some parts of Scotland, and importing a " raw mist," or that deposition of moisture which is intermediate between rain and fog. Etymology, however, is a field for the imagination to sport in. How many words may be found to correspond in sound and signification even in the Hebrew and Scottish languages! But, as words were evidently sometimes intended to imitate the sounds of which they were made the signs, and at other times the sounds occasionally made by the objects they were intended to designate, coincidences are not unlikely to occur, even in languages as remotely connected as these. Thus, in the former, *pech*, (*on the faith of lexicographers*), sig. *expirare*; and in the latter it has exactly the same signification : " *Peching* and groaning like a broken-winded horse." Again, in the Celtic, the name for a sow's trunk or snout is *groin*, which, when well pronounced, exactly resembles the sound produced by means of that organ. The attempt to form words whose sounds resemble, in some respects, and more or less perfectly, the thing or action they are intended to designate, is discoverable in many, if not in all languages; and, (by the aid of a little imagination), we may possibly be able sometimes to discover, how, with this object equally in view, an action shall be expressed, in two different languages, by words whose sounds bear little or no resemblance to each other. Thus, *pluo* in Greek, *spit* in

Instances of the *third* variety of ways in which polished metals may acquire moisture after sunset, are much less familiar than either of the former;—and, indeed, can seldom be observed without some trouble, self-denial, and even risk. Few things of the kind being more injurious to the health of persons accustomed to the usual refinements of life, than lengthened exposure in the open air on such nights as are most fitted for making experiments and observations on the spontaneous condensation of moisture. There can be little doubt, that the persevering and ingenious Dr Wells injured his health not a little by the unwearied ardour with which he prosecuted his favourite pursuit; and that too, according to his own account, under very disadvantageous circumstances.

When a piece of polished metal is placed on grass, whose temperature is already considerably reduced below that of the air at a short distance above the ground, if its size is not considerable, or if the cold of the grass is great, relative to the temperature of the subjacent soil, the piece of metal will also become somewhat colder than the air a short distance above it, and the more speedily, if repeatedly moved to different parts of the grass. Again, if a piece of polished metal be suspended in the air, a short distance above the grass, after, or until the latter has had its temperature considerably reduced, the metal will acquire the temperature of the air in contact with it, and this being colder than the air a few feet from the ground, so also must the piece of metal be colder than the air at that height. Here, then, we have two instances of polished metals becoming colder

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English. Now, there are two ways of ejecting the saliva, the one practised by those who have but little, the other by those who have rather a superfluity of that fluid. If, then, we attend to climate, in as far as that has a tendency to promote the cutaneous more than the mucomembranous discharges, and *vice versa*, and if we apply this to the case of Greece and Britain, we may be led to infer, that one mode [illegible] the saliva would be most common in the one country, and the other [illegible] *[illegible] was first used*) in the other. Hence the origin, perhaps, [illegible] though very different in sound, nevertheless exactly imitate [illegible] ch is Etymology!

than the adjacent air, when exposed after sunset; and that, without the operation of any thing equivalent to radiation, at least in as far as the metal itself is concerned. If now, we suppose the adjacent warmer air to contain, or to acquire, such a quantity of moisture, that a deposition must necessarily take place, if reduced in its temperature to that of the piece of metal, and if we suppose this damp air to be brought, by some mechanical impulse, into contact with the metal, we would expect moisture to appear, obscuring the polished surface. This is exactly what occurs in nature. Even when the air is in its most tranquil state, it is never altogether free of motion, convolving, undulatory or progressive. On the evenings more particularly referred to in these remarks, uncertain local and temporarily progressive motions are not unusual at the lower, and also at the upper, boundary of the stratum of air next the earth. Dr Wells found occasion more than once to refer to this agitation of the lower air, "even in its stillest states;" and, he remarks, that "the quantity of dew seemed to be increased by a very gentle motion of the air." This he accounts for, on the principle, that "a slight agitation of the air, when the atmosphere is pregnant with moisture, will bring fresh parcels of air more frequently into contact with the cold surface of the earth."

A writer in the Edinburgh Encyclopædia * observes, that if the reduction of temperature was produced by evaporation, "the difference between the temperature of the ground and that of the atmosphere near it, would diminish as the air became moist," &c., and that evaporation could have nothing to do with the reduction of temperature "observed on substances exposed in a state of dryness, and not in contact with the earth." If, in the first case, the lower air is understood to remain perfectly at rest on such occasions, and, in the second, that its temperature is the same at various distances from the ground, the conclusion of this writer might be just. But, as neither the one nor the other is the case in nature, his argument seems to have no weight against the paramount influence of evaporation. A very small abstraction of heat will, in certain cases, produce a copious precipitation of moisture; and, on such occasions, if the solid body which

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* Article 'Meteorology.'

has caused the precipitation, afterwards becomes surrounded by a body of air that is not saturated, its temperature may be reduced by evaporation below that of the contiguous air. In the course of nature, however, this can be but a rare occurrence.

As it is impossible to acquire any accurate geological knowledge, by examining the appearances exhibited by a single quarry, mine or cliff, or by several such places, more especially when the locality, &c. is not greatly different, so is it with meteorology, as it regards the phenomena exhibited by the vaporisation and condensation of water; mountain, hill, and valley,—dry plain and marshy meadow,—the sea and fresh-water lakes, rivers and stagnant ditches, must all be familiar, and the appearances there exhibited carefully attended to, before any accurate estimate can be formed of the causes which operate in modifying the spontaneous formation and reduction of aqueous vapour. In this point of view, Dr Wells was unfavourably situated; but he has given us an excellent example of what zeal and perseverance, aided by a masculine intellect, may effect, even in very unpromising circumstances.

On one occasion, in the month of July, during a tract of fine weather, and immediately on sunset, I had an opportunity of witnessing a very interesting exhibition of that motion which takes place in the lower air, at a time when the atmosphere might, by persons not conversant with meteorological pursuits, be considered perfectly tranquil. It was an evening, as described by the poet, when "a solemn stillness reigns." The scene was a perfectly level meadow, destitute of trees, but in which were a few straggling sheep and cows; and it was surrounded on all sides by rising grounds, varying, of small elevation, but rising gently as they receded. The place from which it was viewed was about 50 feet above the level, and within less than a gunshot of the side of the meadow, commanding a complete view of the whole. Suddenly the eye was arrested by a very low white mist, steaming from the whole surface of the meadow. At first it did not extend higher tl [illegible] ers of the sheep, and had throughout a peculiar ind [illegible] motion, resembling small broken waves, not ad [illegible] orizontal direction. In the course of a few minutes, when in depth it reached to the backs of the sheep, and bodies of the cows, its density and whiteness had considerably increased, and its agitated motion had begun to subside. Several large waves now made their appearance, rolling in various directions, and with a velocity that may be described as being neither slow nor quick. By the time its depth had extended to the backs of the cows, only one extensive wave was to be seen, which traversed the whole width of the meadow, moving sometimes in one direction, and at other times in another. When the large wave rolled over the meadow, and had got nearly to the opposite side, considerably accumulated, though its depth diminished backwards to the place from whence it set out, the grass was never perfectly uncovered, and the alternate concealment and exposure, or half exposure, of the sheep and cows thus produced, gave a curious variety to the scene. At last the mist disappeared as it were by enchantment, after having been visible from 15 to 20 minutes. At the instant of its disappearance, a distinctly perceptible motion took place in the lower atmosphere towards the west; but this breeze communicated no apparent motion to the mist, which simply vanished, and in less than five minutes the atmosphere was again nearly as calm as at first. There was, however, no re-formation of the mist, the after-part of the evening was clear, the distant surface, as usual, slightly hazy, and there occurred a considerable deposition of dew. During the rolling of the mist, no such motion of the air would have been suspected by a person walking over the meadow, if the mist had been invisible, or if his attention had not been directed to its movements *.

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* A similar deception in regard to the absence of motion in the lower air occurs during calm hot weather, and on places that are exposed to the full influence of the sun's rays. In walking over a level meadow, on such an occasion, a person might be led to suppose that the air was perfectly at rest; but if he recline on the grass, and look in a horizontal direction over its surface, he will readily perceive not only motion, but a rapid agitation, and that, on some occasions, a considerable distance above the ground. On dry sandy plains, motion near the surface is less apparent, after the ground has become very hot. On some such occasions, may not the plain where the agitation is most considerable, be higher than the person's head, when in the erect position? And is it not probable that the vibratory motion which some have supposed they had discovered in clouds having a fibrous

It is rare to have this motion of the air rendered thus visible on low plains, and so near the surface; but something very much the same may more frequently be seen from mountains high enough to command a downward view of the clouds which form in the evening at the upper boundary of a lower stratum of air, that is incumbent over extensive low plains, in which vegetation is luxuriant.

In a former part of these remarks, similar temporary and local agitations of the air, when otherwise in a calm state, were found perfectly to account, as it is believed, for the increase of temperature indicated by a thermometer lying on snow; and, on the present occasion, it enables us equally satisfactorily to explain the condensation of vapour on polished metals, after sunset, and at a time when hygroscopic and similar substances have suffered a considerable depression of temperature below that of the air a small distance above them.

Two circumstances may here be adverted to, though, after what has been already said, their explanation presents no difficulty: 1*st*, On the occasions referred to, polished metals never have their temperatures much reduced; and the quantity of moisture condensed on their surface from that cause is never very considerable. 2*d*, The surface of a polished metal is sometimes observed to become obscured by the condensation of vapour, and shortly afterwards again brilliant from the re-evaporation of the moisture, at a time when hygroscopic and other similar substances seem to suffer no change in regard to moisture. The least quantity of moisture condensed on a polished surface,

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appearance (if it did not proceed from the unusual irritation which the light reflected from such clouds always communicates to the eye), and that twinkling of the stars so much more apparent at one time than at another, are produced by similar agitations of the air, at no great distance from the earth? Two strata of air, in different conditions, in regard to heat and moisture, often come into contact. On some such occasions a cloud is produced, and on others rain or snow; depending, it is presumed, on the relative conditions of the two bodies of air, and the degree of mechanical force with which they are brought into contact, or blended. But there are doubtless occasions when the conditions of two or more contiguous strata are such that the [redacted] icates heat to the colder, without any deposition of moisture; an ns, such an agitation may take place at the plains of intermi the appearance of a vibratory motion, or twinkling of bodi ce.

particularly if metallic, and a very slight though momentary increase of its quantity, is readily discernible; but it is far otherwise in the case of rough and unpolished surfaces, whether vegetable or mineral. On these moisture is often deposited, and on other occasions evaporated, without our being able to detect the change by ocular inspection *.

5. Glass and lead, bulk for bulk, have nearly the same capacity for heat, and which is about one-half that of water. Glass also is a bad conductor of heat; and, among metals, lead is the worst conductor, platinum alone, perhaps, excepted. When exposed on a clear evening after sunset, glass is sooner dewed than metals; and lead is the soonest dewed of metals, at least of all those that can be readily procured for experiment.

This greater facility of being dewed possessed by glass, has been attributed to its greater radiating power, and, by others, apparently to a greater attraction which glass has for water; air at the same time being understood to be admitted into closer physical contact with glass than with polished metals. A knowledge, however, of the small capacity and low conducting power of glass, seems to be quite sufficient to enable us to account for the difference found to subsist between it and metals, in regard to the disposition to acquire moisture, when similarly exposed in circumstances favourable to that operation.

The principle of radiation has also been introduced to explain the occasional condensation of moisture on the glass of a chamber window, as modified by the operation of an inside and an outside shutter. But to account satisfactorily on this principle for the peculiar forms which the moisture is sometimes found to assume would seem to be rather a difficult task. If, on the other hand, we take into consideration the well known physical properties and mechanical operation of the wood and the glass, in connection with those of the two bodies of air,

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* In the course of these remarks on the relations of polished metals to aqueous vapour, experiments made by myself have been little adverted to, in order to prevent objections as much as possible. I may mention, however, that, in 1812, I had several thermometers constructed, the sentient parts of which were polished metals, and these plates could be covered with gold and silver leaf, copper-foil, tinfoil, and mercury; and it is by means of such instruments alone, perhaps, that accurate experiments of this kind can be made.

so different in their condition, yet not altogether disconnected and which impinge on the opposite sides of the window, the condensation of the moisture, and the forms which it is sometimes found to exhibit, will not be found inexplicable, without having recourse to any thing of the nature of radiation. If the cause of the absence of moisture on certain spots of the glass, (such as are to be seen, Pl. IX. Fig. IV., on the middle and lower pane of the upper sash of the window), be not always very apparent, still the difficulty cannot be removed by supposing the influence of radiation. Unless, indeed, we are satisfied with saying, that some spots of the glass radiated their heat less copiously, or retained more of that which was radiated to them, than the other parts. This would be a very convenient mode of accounting for physical phenomena; for, in adopting it, we would but rarely meet with any very imposing difficulties.

Mr Murray has recorded * an observation made by him when travelling in a coach in Italy, and which he considered inexplicable, excepting on the principle laid down by Dr Wells. The facts, I believe, were shortly as follows: The heat of the external air was 27°, that inside the coach, the windows being shut, 54°. The inner surface of the windows, incrusted with ice, had a temperature of 32°. The outer surface of the glass was dry, and the front windows, shaded by the cabriolet, were free of ice. On lowering the window about half an inch, the crust of ice disappeared very shortly, and the temperature of the air inside the coach was considerably diminished. These facts, however, admit of explanation on a principle different from that laid down by Dr Wells. Before the window was let down the air inside the coach, having a temperature of 54°, would necessarily acquire much moisture from the perspiration and breath of the inmates; while the glass of the window was reduced to 32°, by the constant action of a current of air 5° colder, on its outer surface. The external air being 27° colder than the internal, and having, consequently, a much greater specific gravity, would, on letting down the window, rush in, and, displacing the warm moist air, would become heated in its turn, and thereby have its capacity f [redacted] reatly increased. It would thus be

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of Philosophy.

enabled quickly to dissolve the thin crust of ice, even though its temperature did not nearly equal that of the originally inclosed air. For, air having a temperature of 27°, when suddenly raised 9° or 10° (and it might in this case have risen considerably higher) would be in a state well fitted to dissolve a thin crust of ice. The front windows were warmer, and therefore free of ice, from being shaded by the cabriolet, which would itself be somewhat heated by its inmates, and would prevent the external cold air from directly impinging on the glass. It might also have happened, that the stream of heated air proceeding from the bodies of the drivers and horses, had some influence, and which would necessarily be most effective on the front parts of the coach.

There are a variety of interesting facts and appearances in nature connected with the subject of this paper, which remain to be considered. In accounting for these, the principle of radiation has also been supposed to be applicable, if not essential. It would seem, however, that they admit of explanation, by taking a more simple, and therefore, possibly, more just view of the subject. The operations of nature often appear complex, but, when best understood, are found to be extremely simple. How effectually does this acknowledged simplicity and apparent intricacy often conceal from us the truth? Leading us into some bypath, where a mental phantasmagoria springing up, first pleases, then interests, and ultimately so deceives and blinds, that nothing is believed to possess so much of reality as that which a few passing years, at most, discloses as the veriest "fabric of a vision."

Explanation of the Figures, *Plate IX.*

The aspects of the windows are marked above the Figures, and under them the existing temperatures of the internal and external air, that of the latter being the lowest.

Fig. 1. Observed at Paris. The house was situated on what is considered the highest ground within the gates; the belveder commanding a panoramic view of the whole city. Nothing interrupted the view from the window to the most distant horizon.

Fig. 2. Observed at the sea shore. The house was within thirty paces of high water-mark, and nothing interrupted the view to the most distant horizon. The sky was very clear; the wind gentle and northerly.

Fig. 3. Observed at the same place as fig. 2., the window having an opposite direction, and being about 15 feet from the ground. The latter gradually rose as it receded, so that at the distance of a gunshot, it was higher than the house, which was of three stories.

Fig. 3. Observed in a house situated on the northern verge of Edinburgh, the window being about 20 feet from the ground, and the view in front and to the left uninterrupted. At some distance to the right there was a row of houses, which partially interrupted the view in that direction. The sky very clear,—no clouds,—the wind N. E.,—a gentle breeze. On the outside, and to the under and middle part of the upper sash of the window, was suspended a bent instrument, one-half of which was of metal, the other of glass; and the spheroidal metallic ball on the longer stem, which had a diameter of about two and a-half inches, was two inches distant from the glass of the window. Opposite to this metallic ball, in the line of direction of the wind, there was a somewhat oval shaped spot on the pane of glass, perfectly free of moisture, and this spot had a diameter equal to about one-half that of the ball. On the same level with the instrument referred to, and close to the side of the window, was attached a screen of polished tin-plate, having the form of a half cylinder, and in which were suspended a thermometer and hygrometer. On the pane of glass, immediately above the tin-plate screen, the otherwise regular form assumed by the moisture is obviously modified.

Art. IV.—*Account of the principal Coal Mines in France, and the quantity of Coal which they yield.*

England and Scotland contain the most extensive coal-works that exist in the world. They are there very numerous, being in the direct ratio both of the enormous consumption of Great Britain, and o [illegible] annual exportation. Several of these immense mine [illegible] tion of the greatest moving powers that can be im [illegible] the most simple and most economical means of transport. It is by means of subterranean navigation, by means of canals and sluices lined with iron, and constructed in the very interior of these mines; by means of inclined planes, artfully managed, in which the friction of the carriages is almost annihilated, by plates of cast-iron on which they roll, and which allow them to be left to their own motion for several miles, that the coals are transported even to the place of embarkation; and it is by these economical proceedings, which are a thousand times repeated every day, that the fuel in question comes to be delivered in England to the consumers at a trifling expence.

The Newcastle mines alone, which are in reality the most productive works known, employ more than sixty thousand individuals, and annually produce thirty-six millions of quintals.

France contains no coal-works of so gigantic a nature as those which exist in England; but one would have a false idea of its richness in this respect, were he to judge from the small number of coal-mines that are wrought on a large scale. This apparent smallness depends upon the circumstance that the consumption of coal is very limited, as a deplorable prejudice, and an adherence to ancient custom, have hitherto prevented the use of this combustible in such of our manufactories as consume the greatest quantity of charcoal, the great furnaces.

About forty departments are known in France which contain beds of combustible substances belonging to coal, namely, the Allier, the High and Low Alps, the Ardèche, the Aude, the Aveyron, the Low Rhine, the Mouth of the Rhone, the Calvados, the Cantal, the Corrèze, the Creuze, the two Sèvres, the Dordogne, the Finistère, the Gard, the Upper Rhine, the Upper Loire, the Upper Marne, the Upper Saone, the Herault, the Isère, the Lower Loire, the Lot, the Maine and Loire, the Maucle, the Moselle, the Nicòre, the Nord, the Pas de Calais, the Puy-de-Dome, the Eastern Pyrenees, the Rhone, the Tarn, the Var, and the Vaucluse.

In reality, several of these deposits are nothing more than merely known, and others of them are only wrought to a small extent. However, there are already reckoned in France 236 mines, from which 9 or 10 millions of quintals are annually taken, having a value of from 10 to 11 millions of francs on the spot, a value which rises to 40 millions, at least with regard to the mass of consumers, as the carriage to the place of consumption amounts to three times, four times, and even in some cases to ten times, the price of the coal.

These 9 millions of quintals, which are nothing in comparison of the comsumption of England, which rises to 75 millions of quintals annually *, are furnished by the following mines:

1. Three millions are furnished by the mines of St Etienne, Rive-de-Gier, and the neighbourhood, in which 1400 workmen are immediately occupied, and where there exist 11 steam-engines, 6 hydraulic engines, and 70 machines à molettes ou à chevaux, (analogous to our jack-rolls with spur wheels, and our whim-gins worked by horses.) The formation in which these mines exist, consists of sandstone and slate. The excellent coal which they produce is transported to all parts of France, and even to Genes.

2. Three millions by the works in the Department du Nord, which employ 4500 miners, and in which there are erected 7 horse machines, 9 steam-engines for drawing off the water, and 16 rotation ones, in constant employment for the extraction of the coal.

This country contains the mines of Anzin and Raiane, which are the most considerable in France, and which produce from 200 to 400 metres. These mines are situated in the formation of coal—sandstones, and slates; but they are covered by a great thickness of limestone deposit, the overlying and unconformable strata of which are horizontal.

3. *Lastly*, The remaining third of the mass of coal which is annually extracted in France, comes especially from the mines of Litry, in the Department du Calvados, which employ more than 400 workmen, and produce upwards of 200,000 quintals of coal; of Carmeaux, in the Department du Tarn, which produce more than 100,000 quintals, and employ upwards of 300 workmen; of Creuzot and others, in the Department of the Saone and the Loire, prod [illegible] than 400,000 quintals of coal; of Champagney and [illegible] the Department of the Haute Saone,

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* The Carron [illegible] ne are said to consume 8000 quintals weekly.

the products of which have been considerably increased of late. These mines may be mentioned as examples of works well conducted, and of great importance for the prosperity of the countries in which they are situated.

Then comes the coal deposit of the Lower Loire, which furnishes 6 mines, two of which are situated in the department of that name, and three in the Department of the Maine and the Loire. The whole quantity produced by them yearly is 250,000 quintals of coal, and they employ upwards of 600 workmen.

Then the Departments of the Nièvre and Allier, which have also 5 coalworks. Here the want of channels of conveyance (especially in the Department of the Allier) has hitherto prevented the works from being carried on to a greater extent. This effect is still more sensible, with reference to the coal deposits situated in the midst of the mountains of the centre and south of France. Those of the neighbourhood of Aubin, in the Department of the Aveyron, for example, might, from their extreme richness, furnish the whole of France with fuel, and yet the quantity annually extracted from them is not so much as 10,000 quintals of coal; and even this small quantity is taken from thirty different mills, by superficial works conducted without any rule, and which are continually deteriorating the precious subterranean domain which the soil contains. The want of market also obliges a considerable quantity of small coal to be left at the bottom of the mines, in the Departments of the Aveyron, the Gard, the Loire, and others; and this quantity, which is thus lost for consumption, may be estimated at a twentieth part at least of the total product of the coal-mines of France. —*(See Bonnard, Ingenieur en chef des Mines.)*

Lastly, The Department of the Mouths of the Rhone is the only one that remains to be noticed with reference to the subject in question. Eighteen mines in this Department employ 200 workmen, and produce annually 180,000 quintals of coal.

The selling price of coal varies exceedingly, according to the quality, the facility of working, and especially the abundance of the products, and the extent of the conveyance. Thus, in the Department of the Aveyron, the mean price is only from 35 to 40 centimes the quintal; in the Department of the Loire, the price varies from 30 centimes to 1 franc; in the Department of the Nord, the mean price is 1 franc 27 centimes; in the Department of the Haute Saone, the price rises from 80 centimes to 2 francs 50 centimes the quintal. The reason of so considerable an augmentation is not difficult to imagine; the conveyances are long, and there is no general market.

In general, the small fat coal, and the meagre coal in large pieces, have nearly the same value, and sell at 25 or 30 per cent. less than the fat coal in large pieces.

According to correct accounts, it is estimated that, at present, 10 millions of quintals of coal may be annually extracted in France, which are sold on the spot for 12 millions of francs; which make the average value of the quintal 1 franc 20 centimes, and proves that coal is wrought in an economical manner in France. These works employ immediately 10,000 miners, and a much greater number of individuals for the carriage of the fuel.—(*Annales des Mines, MM. D'Hellancourt and Cordier.*)

The price of coal in France in some of the principal places of consumption is as follows:

At Bordeaux,	large coal of Rive-de-Gr.	5 francs	20 centimes the quintal.
	Carmeaux coal, -	4	20
	Aubin coal,	3	20
At Paris,	St Etienne and Anzin,	4	00 to 4 70
At Nantes,	St Etienne, -	4	30
At Brest,	St Etienne, -	5	30
At Cherbourg,	Litry, - -	4	50
At Rouen,	St Etienne, - -	5	30

Belgium is rich in coal-mines; those of the neighbourhood of Mons, Charleroi, Liege, are very important; they amount to 350, which employ 20,000 workmen, and produce annually about 12,000,000 quintals of excellent coal.

Germany, taken altogether, is not rich in coal-mines; the colleries of the country of Sauebrück, Roer, the county of La Marck, those of the country of Tecklenburg, and the 100 mines of Silesia, scattered in the neighbourhood of Schweidnitz, may, however, be regarded as very important. Lastly, Saxony, Bohemia, Austria [redacted] Bavaria, Hanover, the Hartz, and Hungary, have als [redacted] but of very inferior importance.

In Sweden [redacted] al-mines, excepting in the province of Scania; the [redacted] to be wrought with great vigour.

Norway appears entirely destitute of coal, as well as Russia. It is, however, probable that the great quantity of wood which these countries contain, has hitherto prevented their inhabitants from seeking to become acquainted with the combustible substances which the under-ground strata may contain; and yet some coal-deposits are mentioned as wrought in Siberia.

In Italy, the Appenines contain some trifling coal-mines. In Spain, coal-deposits are known in Andalusia, Estremadura, Catalonia, Arragon, Castile, and the Asturias; but the beds are thin, and the workings are all of little importance. In Portugal there is only one coal-mine mentioned, which is wrought at Cape de Buargos, in the province of Beira. Beds were discovered some years ago near Via-longa, to the north-east of Oporto.

We have few accounts regarding the coal-mines of the other parts of the globe. We know, however, that much coal is wrought in China and Japan; that it exists in the island of Madagascar; that Africa is not destitute of it; that coal has been discovered in New Holland; and, lastly, that it is found in America also. There is little known in the Cordilleras; a deposit is mentioned at Santa Fè de Bogota, which is situated 4400 metres above the level of the sea *. Beds of coal are noticed as occurring at Lucayes, in St Domingo, in the Isle of Cape Breton, in Canada, in Louisiana, and especially in the United States. In this latter country, the whole western part of Pennsylvania and Virginia contains extremely abundant deposits of coal, but which have not hitherto been wrought †. Coal is also mentioned as being found on the coast of Greenland. (*Annales des Mines.*)

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* Probably lignite.

† America has not yet, like the Old Continent, arrived at the point of being obliged to have recourse to its colleries.

Art. V.—*On the Modes of Notation of Weiss, Mohs, and Haüy.* By M. Levy, M. A. &c. Communicated by the Author. (Continued from page 135.)

THE next question to solve, is to determine the laws of decrements, by which the hypothetical forms which have just been considered may be derived from the adopted primitive rhomboid. This may be effected without difficulty, by means of the formulæ I have demonstrated in one of the preceding numbers of the Philosophical Journal of Edinburgh, to discover from certain parallelisms of edges, the indices of a secondary plane. To find the law, for instance, from which may be derived the rhomboid, the superior edges of which correspond to the lines *da*, *dc* of the dodecaedron, it will be sufficient to find the indices of a plane parallel to the diagonal *mn* of the primitive Fig. 2, and also to the intersection *da* of two faces of the dodecaedron. Now, the formula above mentioned, in the case where the secondary plane, whose indices are required, is parallel to one of the diagonals of the primitive, is

$$\frac{P_5}{n_5} = \frac{\frac{1}{n_5\, m_4} - \frac{1}{m_5\, n_4}}{\left(\frac{1}{n_5\, P_4} - \frac{1}{P_5\, n_4}\right) - \left(\frac{1}{m_5\, P_4} - \frac{1}{P_5\, m_4}\right)}$$

In the present case we have $\frac{1}{m_5} = x$, $\frac{1}{n_5} = y$, $\frac{1}{P_5} = z$, $\frac{1}{m_4} = x$, $\frac{1}{n_4} = z$, $\frac{1}{P_4} = y$. Substituting these values, the formula gives, $\frac{P_5}{n_5} = \frac{yx - xz}{(y^2 - z^2) - (xy - xz)} = \frac{x}{y + z - x}$; and therefore the rhomboid assumed as a hypothetical primitive form, may be derived from the primitive rhomboid by a decrement of $\frac{y + z - x}{x}$ rows in breadth on the superior angle. If the quantity $\frac{y + z - x}{x}$ was negative, t of a decrement by $\frac{-(y + z - x)}{x}$ rows in bread r angle. In the same manner, it may be proved, that the rhomboid, the superior edges of which correspond to the lines *db*, *dδ*, results of a decrement by $\frac{x+y-z}{z}$ rows in breadth, on the superior angle of the primitive, and also that the two rhomboids, the oblique diagonals of which correspond respectively to *db*, *dδ*, and *da*, *dc*, result of decrements by $\frac{2z}{x+y}$, and $\frac{2x}{z+y}$ rows in breadth on the superior angle of the primitive; and, lastly, that the rhomboid, the inferior edges of which correspond to the lines *ab*, *bc*, *cd*, results of a decrement by $\frac{x+z-y}{y}$ rows in breadth on the same superior angle. To complete the subject of hypothetical primitive forms, let it be proposed to find the indices of the dodecaedron $(b^{\frac{1}{x}}\, b^{\frac{1}{y}}\, b^{\frac{1}{z}})$, with respect to a rhomboid the sign of which is a^n, that is resulting of a decrement by *n* rows in breadth on the superior angles of the primitive. Let $\frac{1}{x_1}$, $\frac{1}{y_1}$, $\frac{1}{z_1}$, be the required indices. It is obvious that $\frac{y_1-z_1}{x_1-z_1}$ will equal $\frac{\cos\frac{1}{2}(i \cdot i)}{\cos\frac{1}{2}(i:i)}$; and, as the same quantity is also equal to $\frac{y-z}{x-z}$, the following equation will obtain

$$\frac{y_1-z_1}{x_1-y_1}=\frac{y-z}{x-y}.$$

Moreover, the preceding formulæ being all independent of the angle of the primitive, the sign of the rhomboid, the oblique diagonals of which are parallel to *db*, *dδ*, with respect to the rhomboid whose sign is a^n, will be $\frac{2z_1}{a^{x_1+y_1}}$, whilst the sign of the same rhomboid, with respect to the primitive form, will be $\frac{2z}{a^{x+y}}$. But I have shewn in the Number of this Journal for January 1824, that if n' and n'' are the indices of two rhomboids with respect to the primitive, and n''' the index of the second with respect to the first considered as the primitive,

$$n''' = \frac{n' n'' + n'' - 2}{n' - n''}, \text{ or } n'' = \frac{n' n''' + 2}{n''' + n' + 1}.$$

Therefore, in the present case,

$$\frac{2z}{x+y} = \frac{\dfrac{2nz_1}{x_1+y_1} + 2}{\dfrac{2z_1}{x_1+y_1} + n + 1}.$$

By means of this equation, and the one before mentioned, $\frac{y_1 - z_1}{x_1 - y_1} = \frac{y - z}{x - y}$, it will be easy to find the values of $\frac{y_1}{z_1}$ and $\frac{x_1}{z_1}$, in terms of the other quantities, or inversely the values of $\frac{y}{z}$ and $\frac{x}{z}$, in terms of n and x_1, y_1, z_1. They will be found respectively,

$$\frac{y_1}{z_1} = \frac{(y-x)\big((n+1)z - x - y\big) - (y-z)(nx+ny-2z)}{\big((n+1)z - x - y\big)(2z - x - y)}$$

$$\frac{x_1}{z_1} = \frac{(x-y)\big((n+1)z - x - y\big) - (x-z)(nx+ny-2z)}{\big((n+1)z - x - y\big)(2z - x - y)}$$

$$\frac{x}{z} = \frac{\big((n+1)x_1 + (n+1)y_1 + 2z_1\big)(z_1 - x_1)}{(nz_1 + x_1 + y_1)(x_1 - y_1)} - 1$$

$$\frac{y}{z} = \frac{\big((n+1)x_1 + (n+1)y_1 + 2z_1\big)(y_1 - z_1)}{(nz_1 + x_1 + y_1)(x_1 - y_1)} + 1.$$

These formulæ apply only to the case when the faces of the rhomboid a^n, forming the superior solid angle, are situated above or below the faces forming the superior solid angle of the primitive, which is the case when n is positive and greater than 1, or negative and greater than z. But, in every other case, it will be necessary to use other formulæ, because then the angle $(i \cdot i)$ of the dodecaedron with respect to the primitive, corresponds to the angle $(i : i)$ of the same dodecaedron with respect to the rhomboid a . If $\frac{1}{x_1}, \frac{1}{y_1}, \frac{1}{z_1}$, still represent the indices of the dodecaedron r[illegible] the rhomboid a^n, it is easy to perceive that the equat[illegible]ess their relations to x, y, and z, making due at[illegible]ove remark, will be

$$\frac{y-z}{x-y}=\frac{x_1-y_1}{y_1-z_1},\text{ and }\frac{z}{x+y}=\frac{nx_1+z_1+y_1}{2x_1+ny_1+nz_1+y_1+z_1},$$

from which the following values are readily obtained:

$$\frac{y}{z}=\frac{(z_1-y_1)(nx_1+y_1+z_1)-(x_1-y_1)(2x_1+ny_1+nz_1+y_1+z_1)}{(z_1+y_1-2x_1)(nx_1+y_1+z_1)}$$

$$\frac{x}{z}=\frac{(y_1-z_1)(nx_1+y_1+z_1)-(x_1-z_1)(2x_1+ny_1+nz_1+y_1+z_1)}{(z_1+y_1-2x_1)(nx_1+y_1+z_1)}$$

$$\frac{y_1}{z_1}=\frac{(z-y)(nx+ny-2z)-(x-y)(nz+z-x-y)}{(z-x)(nx+ny-2z)+(x-y)(nz+z-x-y)}$$

$$\frac{x_1}{z_1}=\frac{(x+y-2z)(x+y-nz-z)}{(z-x)(nx+ny-2z)+(x-y)(nz+z-x-y)}.$$

Which formulæ ought to be used instead of the four preceding, when n is positive and less than 1, or negative and less than 2.

If, in the two last formulæ, n is supposed to be equal to $-\frac{1}{2}$, they become

$$\frac{y_1}{z_1}=\frac{(y-z)(x+y+4z)-(x-y)(z-2x-2y)}{(x-z)(x+y+4z)+(x-y)(z-2x-2y)}$$

$$\frac{x_1}{z_1}=\frac{(x+y-2z)(2x+2y-z)}{(x-z)(x+y+4z)+(x-y)(z-2x-2y)}.$$

These formulæ, therefore, determine the decrement which should take place upon the rhomboid $a^{-\frac{1}{2}}$, or $e^{\frac{1}{2}}$, to produce the same dodecaedron as that whose sign with respect to the primitive is $(b^{\frac{1}{x}}\,b^{\frac{1}{y}}\,b^{\frac{1}{z}})$. But the rhomboid $e^{\frac{1}{2}}$ measures the same angle as the primitive, and differs only from it as to its position, its oblique diagonals corresponding to the superior edges of the primitive, and its superior edges to the oblique diagonals. Consequently, if a dodecaedron was derived from the primitive by an intermediary decrement, the indices of which were $\frac{1}{x_1}$, $\frac{1}{y_1}$, $\frac{1}{z_1}$? the ratios between $x_1\,y_1\,z_1$ being determined by the two preceding formulæ, it would be equal to the dodecaedron, whose sign is $(b^{\frac{1}{x}}\,b^{\frac{1}{y}}\,b^{\frac{1}{z}})$, but its position with respect to the primitive would be different, since it would be situated relatively to the rhomboid $e^{\frac{1}{2}}$, precisely as the dodecaedron $(b^{\frac{1}{x}}\,b^{\frac{1}{y}}\,b^{\frac{1}{z}})$ is relatively to the primitive. It is obvious *a priori*, from the symmetry of a rhomboid, that two equal dodecaedrons, differing in position, and the principal sections of which are inclined at an angle of 60°, may be derived from the same primitive form, and the two last formulæ determine the indices of one of them, when those of the other are known. If it were required, for instance, to determine the indices of the dodecaedron, similar to the metastatic of carbonate of lime, but differing as to its position with respect to the primitive, it would be sufficient to substitute in the two last formulæ for x, y, z, the indices of the metastatic, which are $x = 1$, $y = o$, $z = -2$, and then the values of $\frac{y_1}{z_1}$ and $\frac{x_1}{z_1}$ will be found respectively equal to $\frac{2}{5}$ and $\frac{4}{5}$, and consequently the sign of the required dodecaedron will be $(b^1\ b^2\ b^{\frac{4}{5}})$, or, according to Haüy's notation, $(E^{\frac{5}{4}}\ B^1\ D^2)$. This modification is one of those he has described, and he mentions other instances of more equal dodecaedrons produced by two different laws of decrements, and I have had occasion to observe several others. There is, however, one case in which the values $\frac{y_1}{z_1}$ and $\frac{x_1}{z_1}$ are found to be respectively $\frac{y}{z}$ and $\frac{x}{z}$; and consequently, in that case, the two laws of decrements are the same, and the two dodecaedrons are not only equal, but their positions are the same, with respect to the primitive. This takes place when $\frac{y-z}{x-y}=1$, or $y - z = x - y$, that is to say, when the dodecaedron is composed of isosceles triangular planes. This last remark proves, that there is an infinite number of dodecaedrons with isosceles triangular planes, produced by intermediary decrements, and that for all these there exists between their indices the following relation, $2y = z + x$.

The last point to be considered, is the determination of the indices of a dodecaedron, when two of its incidences are known. Those which [illegible] are most readily measured, are designated by (*i* [illegible] Three of the preceding formulæ determine imm[illegible]ices of the dodecaedron, when they result from simple decrements on the edges or angles. The remaining case to be examined, is therefore the determination of the indices of a dodecaedron resulting from an intermediary decrement. It has already been proved, that

$$\frac{\cos \frac{1}{2}\,(i \cdot i)}{\cos \frac{1}{2}\,(i : i)} = \frac{y-z}{x-y}.$$

And if another simple relation may be obtained between x, y, and z, the problem will be resolved. It has been proved, that the dodecaedron under consideration may be conceived to be derived from a decrement on the lateral angles of a rhomboid, the oblique diagonals of which correspond to the lines db, $d\partial$, Fig. 1, by $\frac{y+x-2z}{x-y}$ rows in breadth, and that this rhomboid is derived by a decrement of $\frac{2z}{x+y}$ rows in breadth on the superior angle of the primitive. Now, the angle of this rhomboid may be easily determined by means of the measured angles $(i \cdot i)$, $(i : i)$, and the number $\frac{y+x-2z}{x-y}$, which is known since it is equal to $2 . \frac{y-z}{x-y} + 1$, that is to say to $\frac{2 \cos \frac{1}{2}\,(i \cdot i)}{\cos \frac{1}{2}\,(i : i)} + 1$.

For, in the Number of this Journal before alluded to, it is proved that if (P, P) represents the incidence of the faces of the rhomboid, the following equation obtains

$$n = 2 \operatorname{tang} \tfrac{1}{2}\,(e_n : e_n) \cos \tfrac{1}{2}\,(\mathrm{P}, \mathrm{P}).$$

From which it will be easy to find, in the present case, the angle of the rhomboid, the oblique diagonals of which correspond to the lines db, $d\partial$. This angle being determined, the law of decrement by which it is derived from the primitive may be calculated by means of formulæ previously explained; and the index of this decrement being made equal to $\frac{2z}{x+y}$, furnishes a second equation, which, together with the equation

$$\frac{\cos \frac{1}{2}\,(i \,.\, i)}{\cos \frac{1}{2}\,(i : i)} = \frac{y-z}{x-y},$$

is sufficient to determine the ratio between two of the three quantities x, y, z, and the third, that is the indices of the dodecaedron. When the angles $(i\,.\,)$, $(i\,;\,i)$ of the dodecaedron will be known, without knowing, at the same time, its position with respect to the primitive, there will be two answers, for then the rhomboid the oblique diagonals of which correspond to db, ∂d, may be derived in two different ways from the primitive, each will give a different equation, and each of these combined with $\frac{\cos\frac{1}{2}(i\,.\,i)}{\cos\frac{1}{2}(i\,:\,i)} = \frac{y-z}{x-y}$, a set of values of the indices. The method I have just explained to determine the indices of a dodecaedron resulting from an intermediary decrement, will be found very simple in practice, because logarithmic calculation may be used.

The formulæ contained in this and the preceding paper, are sufficient to find the indices of rhomboids and dodecaedrons, when some of their incidences are known. It remains now to explain in what manner their angles may be calculated when their indices are given, and which may, at a future time, be the subject of another communication.

ART. VI.—*Account of the Poison Plants of the Southern Parts of Brazil.* Continued from p. 100.

THE first historians of Brazil have spoken much of the art with which the Indians prepared their poisons. Piso says* they can at their pleasure infect the air and waters,—poison their arrows,—the clothes of their enemies, and even the fruits upon which they may have to feed. But, as Southey † sagaciously insinuates, it is very probable that such tales have been imagined, to gratify the hatred of the oppressors against the oppressed; and the latter, perhaps to make themselves be feared in their turn, may have sought to believe themselves the fables which were originally invented for the purpose of rendering them odious. Piso sufficiently justifies this assumption, when he asserts that the Indians, while they made a mystery of their poisons, readily discl [illegible] antidotes. It is evident, that, if these men were interest [illegible] ging the fatal secrets which are attributed to them, they would have an equal interest in concealing the remedies which might destroy the effect of their poisons. Piso, however, has revealed one of their recipes to us; and we find it composed of a strange mixture of seeds of a leguminous plant, which he names *Mucunaguaçu*, of those of *Cerbera Ahovai* and *Thevetia*, (*Ahovai guaçu* and *miri*); the gall of a toad; the worms which are produced in the juice of manihoc; the leaves of certain sensitive plants, (Herba casta), and those of the species of Rubiaceæ, which he names *Taugaraca*, or *Erva de rato*. If I add to the plants which I have just mentioned the *Annonaæ*, named *Araticu pana*, and the *Sapindaceæ*, which Piso calls *Curuniape** and *Timbo*, we shall have with the manihoc all the poisonous plants mentioned by Piso. Now, we see, that, if some of these plants may, in certain cases, prove detrimental to health, they are very different from those terrible poisons of India, the very idea of which is enough to excite terror. Such vegetables as the *Araticu pana*, which, according to the avowal of the author himself, only causes accidents, when eaten to excess; and the *Herbæ castæ*, of which Marcgraff, although he has figured them, has not even indicated the poisonous qualities, are certainly not of a very formidable nature.

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* Bras. [illegible]

† History of Brazil, vol. i. p. 237.

Aruda and Coster, who have lived in the same country as Piso, since his time, do not take notice of any such plants as those which I have quoted; and in general, they do not make mention of any poisonous vegetable.

I do not doubt, that, in the warmest parts of the south of Brazil, there are found plants whose properties are highly deleterious, of which a proof is afforded by the *Oassacu*, with an inebriating smell, cited by Martius †. But, although the Flora of Fernambucca has a great resemblance to that of the provinces of Santo Spirito, Rio de Janeiro, and Minas Geraes, I am, perhaps, already too far from my subject, in speaking of the vegetation of a country in which I have not travelled; and I shall therefore confine myself to that of the countries which I have actually traversed.

No person was more capable of instructing us with regard to the antient traditions of the Indians, than the famous Father

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* Paullinia pinnata. L.

† Phys. Braz. 11.

Anchieta, who lived so long among them, and who possessed so perfect a knowledge of their language. Yet, besides the manihoc, he does not mention in his letter, upon the province of St Paul, any other poison than that of the *Timboes*, the *Sapindaceæ*, of which Piso, as I have observed, had already cited several species; and which, like the *Coque de Levant*, have the singular property of rendering fishes torpid,—a property equally pointed out by Barrère, La Condamine and Adanson, both in the *Paullinia cururu*, and in the *P. pinnata*.

The Abbe Vellozo de Villa-Rica, who had long travelled in the province of the Mines, with the view of examining its vegetation, has carefully pointed out in his manuscripts the properties of the plants which he had gathered; and the only ones which he mentions as poisonous, are still a *Paullinia* or *Timbo*, which, he says, is fatal to mammifera, and one of his Salviniæ, or *Erva de rato*, a rubiaceous plant, which is the same as one of Marcgraff's *Ervas de rato*, and which is represented as being very injurious to cattle *.

In a general list of the most remarkable Brazilian plants, the Abbe Casal names only one whose properties are deleterious, the tree called *Tinguy* †, the leaves of which, like those of the *Timbo*, kill fishes, and which I have determined to be an anomalous *Sapindacea*. When, afterwards, the same author treats particularly of the vegetation of the provinces which extend between the Rio de la Plata, the Carynbenha, and the Rio-Doce, he still signalizes no other poisonous plants than the *Timboes* ‡, which he then confounds with the *Tinguy*, and a *Guaratimbo*, to which he says the insalubrity of the waters of the *Muryahe* are attributed. He says, indeed, when speaking of the vegetation of the Mines, that poisonous plants are found in that province; but, as he adds that they cause fishes to die, it is plain that it is the *Timboe* which he still has in view.

My respectable friend, the P. Leandro do Sacramento, has pointed out a noxious plant, which he calls the *Martiusea physalodes*; but it appears that he only considers it hurtful to cattle§.

Mawe, Luk[illegible] Eschwegge are not botanists; yet the latter staid for a long time in the province of the Mines; Lukok lived for ten years at Rio de Janeiro, St Catherine, Rio-Grande and S. Joao-del-Rey; and it is to be supposed, that, if these authors had meant to speak of some dangerous poisons, they would have made mention of them in their writings.

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* Palicoure[illegible]

† There are two species.

‡ Cong. t. i[illegible]

§ See Schultes, Mant. p. 226.

In reality, MM. Spix and Martius say, in their interesting travels, that in the neighbourhood of Rio de Janeiro, the *Cancer Uca* retires among the roots of the mangliers, to feed upon poisonous plants; but the learned Bavarians do not name these plants; and as the remark which I have cited occurs only in a note, it is to be believed that it is only the result of a supposition which the authors have conceived, because they considered the crab in question as a suspected animal.

With regard to myself, I have met with many plants in my travels, which, in certain circumstances, and taken in certain doses, might prove very hurtful; some very active stimulants, acrid plants, *Euphorbiaceæ*, which often cause dangerous purgings, &c. I have received confirmations of the properties of the *Timbo* and *Tinguy* (*Magonia pubescens* and *glabrata*, N.); and I have even been assured, that one of the *Timboes* was not only hurtful to fishes; but that it might be dangerous for quadrupeds, as well as for man, (*Serjania lethalis*, N.). Several *Rubiaceæ* (*Rubia noxia*, *Psycotria noxia*, *Palicourea Marcgravii*, N.) have been pointed out to me by the planters; and always under the name of *Erva de rata*, as causing death in beasts that eat of them. The leguminous plant, which is called Jacatupè, and whose roots are edible, is said to produce poisonous flowers. A *Convolvulus*, which I have found abundantly upon the shores of the sea, in the provinces of Rio de Janeiro, and of the Holy Spirit, is also asserted to be dangerous for cattle. A sort of inebriation is produced, when one has eaten to excess of the fruits of the *Myrtea*, which is commonly named *Cagaiteira*. The *Miomio* of the Rio de la Plata destroys horned cattle. It appears certain, that the *Schinus arroeira* causes swellings in those who sleep under its shade. Lastly, I have been assured, that the root of the *Mimosa*, called *Spongia*, was a true poison, &c.

These are undoubtedly dangerous plants; yet, after what has been said above, it is clear that hitherto no poisonous species has been discovered in the southern parts of Brazil, that could be compared, for example, with the *Tieute* or the *Anthiaus upar*; and I would even be led to believe, that there is not proportionally a greater number of noxious plants in this country, than in the Flora of our own.

The plant which renders the honey of the Euxine Sea poisonous, is very far from being a poison of the first order, as is sufficiently proved by the effect, which, according to Guldenstædt's relation, it produces upon goats; and, consequently, the species, whose juices frequently poison the honey of the *Lecheguana* wasp may very well be no more dangerous than the *Azalea pontica*.

It is by no means probable, that it is an *Andromeda*; for I have seen no species of the family of *Ericaceæ* in the province of the Rio Grande, the Cisplatine province, and that of the Missions. It would still less be an *Azalea*, since not only does no plant of this genus grow in the different parts of America which I have travelled; but also of the hundred families that have been indicated by M. de Jussieu in his *Genera*, that of the *Rhodoraceæ* is the only one of which I have never found a species in the course of my travels.

Farther, my suspicions must fall upon a very small number of plants; for the one which had rendered the honey of the wasps of the Rio de Santa Anna poisonous, grew in that district probably only in a very inconsiderable space of land, since, at the distance of some leagues from Rio de Santa Anna, the honey of another nest of the *Lecheguana* wasp was no longer narcotic. It is even pretty probable, that the plant which often renders the honey of the Lecheguana wasp dangerous, does not grow in any part of Old Paraguay; for Azzara, who speaks of the inebriating honey of the bee *Catabatu*, and who has very well described the nest of the *Lecheguanas*, does not say that the honey of these wasps is frequently dangerous. Besides, the same author furnishes us with no data regarding the noxious plants of Paraguay, since, among the pretty considerable number of vegetables belonging to that countr [illegible] he observed on a journey, he does not designate any [illegible] hurtful qualities.

If I now co [illegible] t work of M. De Candolle, upon the medicinal [illegible] nts, and the best authors who have written upon the same subject, and join to their observations the fruit of my own researches, I shall find, that the number of the families of phanerogamous plants, that produce narcotic species, the only ones which should naturally engage my attention, reduces itself to twenty, namely, the Menispermæ, Sapindaceæ, Papaveraceæ, Terebinthaceæ, Leguminosæ, Rosaceæ, Umbelliferæ, Cichoraceæ, Rhodoraceæ, Apocineæ, Solanaceæ, Scrophularineæ, Euphorbiaceæ, Coniferæ, Aristolochiæ, Irideæ, &c. Casting a glance upon the species which I have collected in a space of about 45 Portuguese leagues, from Belem to the Ibicuy, a space in which the Rio-de-Santa-Anna flows, I only find plants belonging to six of the above families, namely, the *Euphorbiaceæ*, (*Euphorbia papillosa, Microstachys ramosissima, Caperonia linearifolia*, N.); *Apocineæ*, (among others the *Asclepias mellodora*, and *Echites petræa, N.*); one *Sapindaceous* plant, *Solanaceæ, Leguminosæ*, and two *Scrophularineæ*. It is, therefore, to these plants, twenty-one in number, that my conjectures must refer; but, as the *Leguminosæ, Euphorbiaceæ*, and *Apocineæ*, do not belong to the genera among which narcotic plants have been peculiarly designated, I shall confine my search principally to the four *Solaneæ* (*Nicotiana acutiflora, Solanum guaraniticum, Fabiana thymifolia, Nierembergia graveolens, F.*); the single *Sapindacea* (*Paullinia australis, N.*), the two *Scrophularineæ* (*Stemodia palustris* and *gratiolæfolia, N.*); and of these it will be upon the *Sapindacea* that I shall make my suspicion chiefly fall, because I already know the narcotic effects which several vegetables of the same family produce in these countries; and because the species which I have signalized was of all those which I have mentioned, that which flourished nearest the wasp-nest the honey of which was so nearly fatal to me.

I cannot close this account, without adding some observations which are not without importance. Dr Benjamin Smith Barton thinks that the poisoned honey injures the bees themselves; but this is by no means probable, or at least it could not do so to them in the same degree as to man. This honey, in fact, has been sucked by the bees; it has resided in their intestines; they have only collected it, by returning a thousand and a thousand times to the same flowers; and if it could prove hurtful to them as to man, it is impossible to conceive that they would have stored it up in their cells.

The American author, whom I have just cited, regrets his not knowing what remedies should be employed in cases of poisoning by honey. Of the three persons poisoned near the brook of St Anna, the least affected vomited after eating; and, it was not until I had vomited myself, that I felt sensibly better. If one of the two herds mentioned by Seringe died, after having eaten honey sucked from Aconitum Napellus and Lycoctonum, he was the one who had not been able to vomit. It is therefore very evident, that an emetic which should quickly rid the stomach of the cause of the evil would be the best remedy to which recourse could be had.

ART. VIII.—*On the Structure and Nature of the Spongilla friabilis.* By ROBERT E. GRANT, M. D., F. R. S. E., F. L. S., M. W. S., &c *. Communicated by the Author.

THE *Spongilla friabilis* of Lamarck, belongs to a genus of organized bodies, whose internal structure and economy are still unknown, and which naturalists are at present undecided whether to place in the animal or in the vegetable kingdom. It is a fresh water production, of a green or grey colour, soft, fibrous, reticulate, friable texture, irregular flat spreading form, and strong fetid odour; it contains a turbid green-coloured gelatinous-like matter in its interstices, and erect branched fibres pass through its interior, arising from its base, and projecting from its surface. Lamarck has distinguished this from the only other known species, *Sp. pulvinata* and *Sp. ramosa*, chiefly by the marked appearance of these erect or longitudinal fibres, which are seen in dried specimens, rising, branching, and radiating towards the surface, and beyond it.

This animal or vegetable production is found spreading on rocks or other solid bodies, at the bottom of lakes, or on the sides of stagn [illegible] ls, and has been observed in various parts of Europe,— [illegible] Pallas,—in Denmark by Muller,—in Sweden by [illegible] Germany by Gmelin, Blumenbach, and Schweigger,—in different parts of Great Britian and Ireland,—in France by Lamouroux; and probably it has not been looked for on other continents. It grows abundantly in Lochend near Edinburgh, where I have procured all the specimens for the experiments and observations detailed in this memoir; it is seen covering the surface of many of the rocks and stones on the east side of the lake, and enveloping the wooden posts at the north end of it, when the water is low in autumn; it spreads indiscriminately over every solid body it encounters, whether animal, vegetable, or mineral, and adheres so closely to them, that it cannot be separated without laceration. We observe it more frequently, and better developed, on the overhanging or perpendicular sides of solid bodies than on their acclivities or their summits; this has relation to the position of certain large orifices on its surface, to be noticed hereafter. Though of a very delicate and brittle nature, it thrives on the most exposed ridges and prominent angles of rocks, which is probably owing to its usual depth from the agitated surface, and to the sheltered condition of small lakes, compared with the open sea, where the marine sponges thrive best on the sheltered sides of rocks. When young, it appears in small, round, convex spots, of a light grey coloured, soft, downy, substance, adhering to the surface of stones under water, or spreading irregularly as a flat woolly covering of a light greenish-grey colour, having a line or two of thickness, and an extension of one or two inches. But as it advances in growth, it becomes more compact in texture, and of a darker sea-green colour, acquires a thickness of more than two inches, covers a continuous surface of several feet in length, sends up from every part of its surface irregular, short, compressed lobes, sharp ridges, thin laminæ, or cylindrical, small branches, rounded at their extremities, and it presents numerous very distinct apertures, of different sizes, leading into its interior. From the looseness of its porous surface and internal texture, and from its mode of enveloping substances in the progress of its growth, we generally find in its interior portions of sand, mud, or gravel, shells of fresh water testacea, fragments of roots or branches of trees, tubulariæ, larvæ, particularly of phryganeæ, innumerable animalcules, and different kinds of ova.

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* R [illegible] rian Natural History Society.

In its living state, the *Sp. friabilis* is so soft and brittle that it can scarcely be handled or lifted without tearing, feels slightly unctuous between the fingers, has a strong disagreeable smell, like that of stagnant ditches in the heat of summer, tastes cooling without any marked flavour, and quickly diffuses among the saliva, leaving only some earthy particle between the teeth; it sinks slowly in water, appearing lighter than most marine sponges. When pressed, a thin slimy turbid greenish-coloured matter escapes, mixed with a considerable portion of water, and the remaining fibrous portion has a light grey colour, and stiff gritty feel. When allowed to putrefy in water, a thick, fatty layer covers the surface of the fluid, the water acquires a turbid yellowish colour, the spongilla becomes of a blackish-green hue, and emits a most offensive putrid animal odour, like that of the most putrid offals. A portion of it, whether fresh or putrid, placed on a red hot iron, smells like burning skin or membrane, the soft parts are dissipated, and the fibrous residue becomes red hot, but does not consume nor change much its form. The burnt remains of this substance do not effervesce in vinegar, nor in nitric, sulphuric, or muriatic acids, nor is their appearance in the least altered by these acids, although they are alleged by Lamouroux to contain more than half their bulk of lime. When the calcined remains, or even a portion of the fresh spongilla, are rubbed with a smooth, wooden, instrument on the polished surface of glass, they leave innumerable very minute permanent traces, which we observe, with the assistance of a lens, to be distinct streaks cut in the substance of the glass, thus indicating the presence of silica in the axis of this organized body. The soft green coloured matter contained so abundantly in this substance, in its living state, when mixed with water, and examined under the microscope, is found to consist almost entirely of minute, granular, transparent bodies, like the gelatinous matter of the marine sponge. The dried fibrous axis becomes of a pure white colour, and somewhat opaque, by a few minutes' exposure to the intense heat of the blowpipe, but does not melt nor lose its fibrous appearance; when a portion of the dried axis is rubbed [illegible] back of the hand, it excites an itching pain, and infla [illegible] diffused redness, from its sharp spicula piercing [illegible] emaining in its substance. Numerous, small, yellow, globular bodies have been frequently observed in autumn, spread every where through the substance of the spongilla, and have greatly perplexed naturalists,—some considering them as the grains of this supposed plant, while others regard them as ova deposited there by some aquatic insects.

Linnæus, in his *Flora Suecica* (1190–1191), speaks of grains found in this fresh water plant in autumn, though in his later works he seems to consider these grains as foreign bodies, and the spongilla as a species of sponge. Lamarck and the Danish naturalist Vahl, considered the spongilla to be merely a habitation constructed by the cristatella. Montagu, in the *Wernerian Transactions*, considered it as a nidus formed by some aquatic insects, for the reception of their ova. Lichtenstein, in the *Trans. of the Nat. Hist. Soc.* of Copenhagen, describes it as an agglutinated mass of the tubes of fresh-water tubulariæ, remaining empty after the death of the polypi. Pallas speaks of it as a shapeless mass, possessing no trace of life. Gmelin, like most of Lamarck's predecessors, places it in the genus *Spongia*, and he makes the singular remark respecting the *friabilis*, that it serves as food for fishes. Lamouroux, in 1816, was satisfied, from personal examination, that it is an animal resembling the group of true sponges; but in his *Expos. Method.* 1821, he expresses himself convinced, from more recent observations, and particularly from the effects of light, heat, moisture, and air upon it, that it differs entirely from the marine sponge, and is merely a fresh-water plant. Lamarck still considers its animal nature as far from being established, and has removed it to a great distance from the marine sponge. In this country, some naturalists, as Dr Fleming, regard it as an animal distinct from the sponge, while others spend their ingenuity in endeavouring to prove it a vegetable. Schweigger has examined two species of spongilla alive, *Sp. pulvinata* and *Sp. ramosa*, and states that they possess a gelatinous crust, as distinct as that of many marine sponges, and truly belong to that genus of animals; while Blumenbach, who has performed many experiments on these substances in their living state at Göttingen, has not been able to discover a trace of animal nature in them, and believes them to be aquatic plants. But none of these writers have described to us its internal organization, nor afforded sufficient data to enable us to decide either as to its animal or vegetable nature.

The small, yellow, globular bodies observed by many naturalists in the *Spongilla friabilis* in autumn, are distinctly visible to the naked eye, regularly spherical, about the size of grains of sand. Linnæus compares them in size to the seeds of thyme, of a bright straw-yellow colour, rough on their external surface, yielding a little to pressure, and quite elastic. I have found them present, and almost equally abundant in the spongilla in September, October, November, December, January, and February, but have not yet examined this substance in other months, to discover at what season, if ever, they are deficient. They are distributed very irregularly, but abound most in the deeper parts, where they frequently lie loosely collected in groups of about twenty or thirty; they have no perceptible organic connection with each other, or with the substance in which they are imbedded. I have frequently found a portion of spongilla crowded with them, while another growing beside it contained none; and even the same portion sometimes presents them crowded in one place, while they are entirely wanting in another. They seem to have no proper cell or particular disposition of the spicula for their lodgement, but fall out readily when the broken substance is moved gently in water; and there appears to be no open passage leading to them from the surface, different from the canals natural to this organized body. When one of these round balls is pressed between the forceps, it yields with some resistance, bursts suddenly, and a white, semi-opaque, viscid matter is forced out. They produce no effervescence when thrown into nitric acid, no lime being contained in their tough cartilaginous capsules; the capsules frequently burst after remaining a minute or two in this acid, being contracted by it, like other horny or cartilaginous substances, before they dissolve. The yellow, elastic capsules, viewed separately through the microscope, have a coarse, granular structure, and appear studded with [illegible]rent points, as if porous, but nothing is perceived to [illegible]h them by pressure, till they burst. In bursting, [illegible] times observed the fluid contents force out a [illegible] portion of the capsule. When these yellow [illegible]osed for a minute or two to the flame of a candle, they diminish to a third of their usual size, become quite black, shining, and smooth on the surface, empty within, and very brittle; this was observed before the time of Linnæus. In this calcined state they produce no effervescence, and undergo no change in the strongest acids.

The soft matter contained within these yellow spheres, consists of two or three hundred soft transparent gelatinous globules, adhering slightly together, and, when magnified by the microscope, very much resembling the spawn of a frog; there is likewise a small quantity of a thin colourless fluid, and some lively monades, as we find within the ova of most animals, but not, as far as I know, within the seeds of plants. When shaken gently in water, or allowed to remain a few minutes in it, the transparent globules fall separate, and begin to dissolve; on examining them with the microscope when thus separated, we observe that each globule contains about a hundred very small white opaque particles, which lie close together on one side of the globule, and occupy about a third of its capacity. The transparent part of the globules quickly and entirely dissolves, and the white opaque bodies they contained are observed strewed over the bottom of the water, partly adhering in groups, and partly isolated. I have not observed any change in these white particles, after preserving them some time in water, though they seem to possess the power of slowly changing their positions, when attentively watched through the microscope.

The yellow spheres whose contents have been described, did not undergo the slightest perceptible change in external appearance, or in the nature of their contained matter, during six weeks rest in rain water, frequently renewed, from the middle of October to the end of November, although the true ova of the spongilla were growing and spreading on watch-glasses immersed in the same vessel of water. And what appears a remarkable circumstance, whether these bodies be ova or grains, their colour, size, structure, and contents, were precisely alike, during all the six months I have yet been able to examine the spongilla alive;—those taken from the spongilla in February presented the same appearances, externally and internally, as those of September. They differ from the ova of every marine sponge I have yet observed, in their strong cartilaginous capsule, and soluble, gelatinous globules; they differ entirely in colour from the substance in which they are found, the spongilla being of a deep sea-green or grass-green colour, while they are of a lively straw-yellow; and they do not develope themselves into young spongillæ, as some would lead us to suppose, in the same circumstances which evolve the true ova of that animal. Different kinds of these bodies appear to occur in the fresh-water sponge Linnæus describes them as shining, bluish globules, about the size of a grain of thyme, in the Spongia lacustris (*Spongilla ramosa*, Lamarck), and as green gelatinous grains in the Spongia fluviatilis (*Spongilla pulvinata*, Lamarck). Lamarck states, that small, yellow, gelatinous grains are found in all the species. Those found in the *Spongilla friabilis* of Lochend are tough, hard, yellow spheres, filled with transparent, soluble, gelatinous globules. Lichtenstein considered them as the ova of the *Tubularia sultana*, Blumenbach, as appears from Schweigger's account of his MS., although he is represented by the French writers as having mistaken them for the germs of the cristatella. From the doubtful nature of these bodies, and their appearing in the same state of development for at least six successive months, their existence in the spongilla cannot with propriety be adduced in proof of this substance being a plant, as is done by Lamouroux and others, nor to prove it an animal, as was formerly done by Lamouroux, and is at present by Lamarck.

The external surface of the spongilla, like that of the marine sponge, is covered with numerous, open *pores* leading into its interior. The pores are mentioned by Linnæus and Gmelin in two of the species, *Sp. lacustris* and *Sp. fluviatilis*. They are so conspicuous on the surface of the *Spongilla pulvinata*, that Lamarck has introduced them into the definition of that species. On the surface of the recent *Spongilla friabilis* they are visible at the distance of twenty inches, and are quite distinct from the large apertures seen between the lobes and branches, which have probably alone been observed. They are distributed irregularly over the whol[illegible]ce, and are surrounded by projecting, naked fibres, ve[illegible]his species. They appear open, round, and smo[illegible]ins, though they are easily obliterated by han[illegible]e substance, or by the natural collapse of their very soft margins. By placing a thin layer, cut from the surface under the microscope, we perceive that each pore, besides its projecting defending fasciculi, has its margin supported by loose spicula lying parallel with the surface, and placed round the opening. The bounding fasciculi of the pores consist of so few spicula, and these are so loosely connected together, that the whole surface wants the compactness which they produce in the marine sponges. These openings are not the cells of polypi, nor can we discover by the microscope any trace of ciliæ on their margins; but their whole internal parietes are closely covered by the same minute, granular bodies which line the pores and canals of the marine sponge; and on viewing these bodies sideways, we observe that they project from the margins towards the centre of the openings, more distinctly than in most of the latter zoophytes. By examining their horizontal sections taken successively from the same part of the spongilla we discover that its pores are only the open entrances to canals which meander through the body, enlarging in their diameter as they proceed, till they again reach the surface. The wide extremities of the canals are the fecal orifices, which are seen of uncommon magnitude, opening on the depressed parts of the surface between the lobes. The granular bodies which line the whole of these canals from the pores to the fecal orifices, are connected with each other, and with the parietes, by means of a very soft, transparent, green-coloured, glistening matter. There are obviously fewer granular bodies on the surface of this gelatinous matter at the fecal orifices than elsewhere; and when we examine it with highly magnifying powers in that situation, it appears quite homogeneous, without fibre or grain in its texture. The internal canals are every where bounded and supported by the longitudinal fibres, and by single transverse spicula, which pass across from one fasciculus to another; at the extremities of the canals the projecting, erect, longitudinal fibres have a slight convergence, both around the pores and fecal orifices. The single transverse spicula which bind together the longitudinal fibres, are almost invisible to the naked eye; hence in dried specimens of the *Spongilla friabilis*, the whole skeleton appears to be composed solely of longitudinal fasciculi, rising from the base, and branching towards the surface. These two kinds of fibres are connected with, and almost imbedded in, the glistening matter lining the canals, and they assist, by their natural curvatures, in giving a roundness to these passages. The fecal orifices, in this species, are never raised to the extremities of projecting papillæ, and have no regularity ot form, size, or distribution. They may be compared with those of the *Spongia panicea*, preferring to open on the deeper parts of the surface; and, like that sponge, this substance thrives best where its free surface hangs down in a vertical position, as when it spreads on the overhanging sides of rocks, or on the under surface of wooden planks.

From this striking resemblance in structure and general appearance between the spongilla and the marine sponges, a resemblance which probably I would never have detected in this soft substance, without adopting every precaution which experience had shewn to be necessary in the examination of the latter zoophytes, I was naturally led to expect the same currents through its internal canals which are so obvious and well known in the true sponge. The shaking of this brittle zoophyte in carrying portions of it from the lake to be examined under the microscope in my apartment, injured so much the organization of its soft parts, as to baffle my first attempts to discover its currents. At length, however, I succeeded by examining portions of it on the side of the lake, the instant they were cut from the rocks. On placing an entire portion of it perpendicularly in a glass of clear water, and in perfect rest, I observed, with a lens, through the sides of the vessel, not only particles of matter driven with rapidity from the large openings between the lobes and ridges, but likewise floating particles distinctly drawn in through the lesser openings, distributed on the elevated parts of the surface. I afterwards succeeded several times in preserving such portions of it as had lobes or branches projecting from their surface, so entire as to exhibit their currents in my apartment for nine hours, after their removal from the rocks. On cutting off these uninjured lobes, and placing them successively under the microscope in a watch-glass with rain-water, I observed the same regular anc [illegible] eams from the small fecal orifices placed at different [illegible] their surface, the same feculent matter accomp [illegible] ns, and the same motionless state of the mass di [illegible] which are observed in the marine sponges. The pores of the lobes are nearly as large as their fecal orifices, and currents are as distinctly seen flowing into them as from the latter openings. I have not been able to excite this substance to any kind of spontaneous motion, and Blumenbach seems to have been as unsuccessful with those he experimented on at Gottengen; nor have I found any difference of temperature between it and the medium in which it lives.

The fibres forming the axis of the *Sp. friabilis* consist of minute siliceous spicula, which are as regular and constant in their forms as the ultimate crystals of a mineral, or the spicula of other zoophytes, and might, like these, be employed to distinguish known species, or to discover new. When we examine a thin layer of the recent spongilla under the microscope, we observe the spicula placed like a frame-work round all the openings, in the order best calculated to prevent these passages from changing their dimensions. By agitating a portion of it in water they fall asunder, and may be procured separate from the soft parts, but not in so pure a state as when they are obtained through the medium of acids. On allowing a portion of spongilla to remain for a short time in a watch-glass with nitric, sulphuric, or muriatic acid, the animal matter dissolves, and the siliceous spicula cover the bottom of the glass like minute shining crystals. They may now be washed, and their symmetrical forms examined under the microscope; or they may be dried between plates of glass, or thin scales of mica, and thus preserved for examination or comparison at any future period. In this species, the spicula have all the same form, and are mostly of one size. From this circumstance, and from the well-marked characters of the *Sp. friabilis*, and its abundance in most inland countries, its spicula may be adopted as a convenient and fixed standard of comparison for the description and measurement of the spicula of every other zoophyte.

They are transparent, colourless, cylindrical, very slightly and regularly curved, pointed at both ends, tubular, hard, and brittle. They scratch glass, suffer no change in nitric acid, become inflated like a bottle, and burst by the sudden action of the blowpipe; do not alter their forms in the least by drying, and do not consume by heat. In their moist state they have a shining, vitreous lustre, and appear through the microscope as if solid and homogeneous throughout; but, on being heated or dried, they lose their lustre, become less transparent, and of a greyish-white colour, and a distinct cavity is observed within them, extending from one point to the other, and occupying about half of their diameter. From the appearance of the sharp points at the extremities of their axis, and from their bodies inflating and bursting by sudden heat, their internal cavity seems to be completely closed at both ends; and from the homogeneous and solid appearance of the spicula in their natural state, they seem to be then filled with a soft matter, decreasing in density from the circumference to the axis, which may contribute to their strength and flexibility. When we place any object, measuring half a line in length, among these spicula under the microscope, we perceive that it requires four of them to extend the same length as that object; thus shewing each spiculum to be the eighth of a line, or eightieth of an inch in length, and their diameter measured in the same way, is about the fourth of that of a human hair. As the spicula of this zoophyte are of a middle size, between the large and the minute, their dimensions might be assumed as unity in the measurement of other spicula; and from the constancy of the forms, and dimensions of these elementary parts of the skeleton, their description would form an important character in the definition of every zoophyte possessed of spicula.

Each longitudinal fasciculus, which appears to the naked eye as a single fibre, is composed of about ten spicula adhering closely together in a body, a like number being added to their extremities to an indefinite length. These spicula adhere to each other throughout their whole length, and are not easily separated by agitation, or by repeated maceration in hot-water; but their connecting matter is quickly dissolved in strong acids, which might lead us to believe, that it differs from the common gelatinous matter of the spongilla. The waving direction of these fasciculi is produced by the curves of one set of spicula being turned opposite to the curves of the next adjoining, and so on in a continued series. [redacted] transverse spicula, which connect the longitudinal [redacted] generally pierce completely through these strong [redacted] a firmer adhesion. The forms and nature of [redacted] and the general construction of the skelet[redacted] found to be the same, whatever might be the external appearance or age of the spongilla, or the part of the lake from which it was procured. The curves of the spicula have a relation to the rotundity of the canals and openings, and their sharp points relate to their function of defending these passages. The whole arrangement of the spicula, around the canals, shows that these are not accidental passages, formed by worms or aquatic insects in a vegetable substance, and helps to prove, that its currents are not produced by any foreign intruders, though this substance is infested with myriads of ciliated animalcules, which are constantly producing currents to attract their prey. In place of the phosphate of lime of the higher orders of animals, or the carbonate of lime of the lower orders, we have seen that silica is the earthy matter of the skeleton of this zoophyte. The same is the case with most of the British marine sponges, and with some zoophytes which possess polypi. This earth is secreted by many plants, but I am not aware that it has been observed in the form of symmetrical, tubular spicula, composing the axis of any substance in the vegetable kingdom.

By a little agitation in water, the gelatinous matter of the spongilla resolves itself almost entirely into minute, pellucid, green-coloured granules, which have a singular tendency to reunite. When allowed to remain for a few hours at rest, they unite into a compact, dark green, velvety membrane, perfectly resembling the *Oscillatoria viridis*, Vauch. and attach themselves to the bottom of the vessel. When a few of them are placed in a watch-glass with water, they form themselves into minute spheres, being constantly rolled to and fro by the animalcules, from which it is nearly impossible to free this substance. The minutest of the granular bodies, when viewed through the microscope, are seen to have a distinct power of locomotion. Their slow motions, in this separate state, are probably produced by the same organs which they employ to produce the currents, when attached to the sides of the canals. The soft matter of the spongilla does not seem to possess a distinct membranous coat, but is a little more consistent, and has a glistening surface, wherever it is in contact with the element in which it lives, as within the canals, and on the outer surface of the body. We observe minute portions of the gelatinous matter assuming naturally a spherical form, within the living spongilla, in the parenchymatous soft substance, between the internal canals. They appear to be the ova or germs of this substance,—they contain no spicula,—and the microscope detects nothing in their structure but transparent granular bodies, like those lining the canals, connected together by gelatinous, homogeneous matter. During October and November, several of these spherical, translucent, greyish-green coloured globules, attached themselves to the bottom of watch-glasses, in which I had placed broken portions of spongilla, and when fixed, they spread, and exhibited the same phenomena of growth, presented under similar circumstances by the ova of the marine sponge. They are not quite so large as the yellow cartilaginous balls of the spongilla, above described; and, when they first lose their spherical form, and begin to spread on the glass as a thin, transparent film, we distinctly perceive, even with a single lens, that they contain no spiculum. With the microscope we can observe the position, size, and form of each spiculum, as they successively make their appearance in the spreading circular film. The spicula first formed were generally two or three, lying close and parallel to each other, and extending from the centre towards the margin of the ovum. Afterwards, I observed single spicula make their appearance, quite isolated, in different parts of the ovum, and often at right angles to the radius of the place where they lay. The radiating double spicula are probably the beginnings of the longitudinal, erect fascicula; and the others the single transverse spicula. The spicula first formed in the ovum have the same form as the adult spicula, and appear greatly disproportioned to the small size of the ovum. I have never observed a spiculum enlarge by growth, after being once formed. The ovum, in spreading, changes its circular form for an oblong or irregular outline, but its spreading margins are always surrounded with a very thin homogeneous film, while its granular bodies and spicula occupy chiefly the convex middle part. I have observed, however, spicula quite isolated make their appearance in the spreading marginal film. None of the spicula are ever observed to shoot their points natu [illegible] the surface, or beyond the margin of the ovum; [illegible] light agitation of changing its water from time t [illegible] ses many of them, already formed within the ovum, to project beyond its surface. This renders it probable that all the spicula, even the naked groups, projecting round the pores and orifices, were originally formed within the surface of the soft matter. Analogy leaves no doubt, that these ova or spherical portions of gelatinous matter, when ready to separate from the parietes of the canals, are delivered by the currents through the large fecal orifices as in the marine sponges; but I have not detected any ciliæ on their surface, nor seen them swim about by their own spontaneous motions, like many marine ova, before fixing themselves. The ova were nourished only with rain water, while the spicula were successively forming in their interior; which shows that these simple gelatinous globules, in which neither vessel nor fibre are discernible, have the power of secreting siliceous tubes from that pure element.

The *Spongilla friabilis* has thus a close resemblance to the marine sponge in its siliceous spicula, gelatinous matter, granular bodies, pores, internal canals, fecal orifices, currents, feculent matter, and general mode of growth, whether in the state of an ovum, or in the adult state; and, as the transition from the sponge to the Alcyonium by a new genus has been shown elsewhere, we have thus a regular and beautiful gradation from this simple substance, to the most complex polypiferous zoophytes. Although in every respect a sponge, it has a more imperfect structure than any of the marine species, which is observable in the sameness and feeble attachment of the spicula, in the great size and defenceless state of the pores and fecal orifices, in the general looseness of its surface and internal texture, —in the softness of its gelatinous matter,—in the want of ciliæ and spicula in its ova, indeed in every individual character. From this greater simplicity of structure, we are forced to consider it as more ancient than the marine sponges, and most probably their original parent; and, as its descendants have greatly improved their organization, during the many changes that have taken place in the composition of the ocean, while the spongilla, living constantly in the same unaltered medium, has retained its primitive simplicity, it is highly probable that the vast abyss, in which the spongilla originated and left its progeny, was fresh, and has gradually become saline, by the materials brought to it by rivers, like the salt lakes of Persia and Siberia. The want of contractile power in this zoophyte, and the absence of all organs for seizing prey, show that it is nourished only by the particles of organic matter suspended in water, or by the elements of that fluid, which is further indicated by the constant streams through its body, and by the development of its ova, when supported only with rain water. The great looseness and softness of its texture, and the width and defenceless condition of its openings, which now render the spongilla a safe retreat, and a convenient magazine of food for myriads of animalcules and aquatic insects, and a fit receptacle for their ova, obscurely indicate the unpeopled state of the waters of the globe, and consequent absence of these numerous assailants, at the period of the first formation of this zoophyte; and its aptness for secreting silica, and the abundance of that earth in its skeleton, show the period of its creation to have been nearly synchronous with that of the siliceous or primitive rocks.

ART. IX.—*General Reflections on various important subjects in Mineralogy.* By FREDERICK MOHS, Esq., Knight of the Order of Civil Merit, Professor of Mineralogy at Freyberg, Fellow of the Royal Society of Edinburgh, of the Wernerian Natural Society, &c. (Concluded from p. 28.)

THE natural-historical resemblance of several species consists in their greater or less agreement in regard to their natural-historical properties. In order to find out this agreement, we must consider the species as *wholes* (which they are, according to the general idea developed above), and not in single varieties, but as complete as possible; in the same manner in which the botanist and zoologist have to compare the complete species of plants and animals, before they can judge rightly of the genus. Thus, a representation is produced, in which all the single connexions of certain natural-historical properties to be met with in individuals in some respect disappear, and are melted together into a kind o [illegible]. This *original* representation of the species, as it [illegible]s different from the idea of the species, which o [illegible] the species are, and also different from the character and the general description. It is not capable of being analysed, or reduced to single characteristic terms or marks; and hence it should be considered only as a whole, taking it in its general compass. An example, taken from the species of Man, which is necessarily familiar to every body, may serve to illustrate this. If we speak of man in general, we do not reflect upon any individual, or a single relation of size, colour, countenance, &c.; nor upon the European, the African, &c.; still less upon the Englishman, the German, the Frenchman, the Spaniard, &c.; but, as it were, upon a mean term of them all, which can never be represented by any individual, but necessarily requires the whole species. In like manner, the representation of the species of Augite (paratomous augite-spar), does not apply solely to diopside, or to augite, or sahlite, or omphazite, or to any other particular variety, but to the whole species, which can never be observed as a single body in nature. We may easily conceive that these representations are not exactly the same in every individual; nay, that there are not perhaps two people that possess them precisely similar;—for who could determine this point? They must therefore be different from the idea of a circle or a square, which are not different in any two individuals. But nothing depends upon this perfect equality, because there are resources in Natural History that are independent upon this difference of conception in different persons, not only for deciding in what species a particular individual should be included, but also for producing the general conception of the species, and of which we shall have occasion to speak more at large. One thing only remains to be observed in this place, which is, that these representations of the species cannot be obtained by way of abstraction,—for, by that process, every thing would either be lost, or, at least, so little would be left, that it could be of no further service in Natural History.

The comparison, in regard to the natural-historical similarity, must now be referred to these original representations or conceptions of the species. If these are found to coincide to a certain extent, and in the highest degree distinguishable, in two or more species, then these species form a Genus, or belong to a genus, of which there is now formed another representation of the same kind, having only a greater extent. The mineral kingdom contains many well-known examples of genera. Those who may compare the original representations of the augite (paratomous augite-spar) with the hornblende (hemiprismatic augite-spar), will find them to agree so very nearly, as to render it often necessary to examine certain particular characters, before it can be discovered to which of the two species the varieties belong; although this is a subject which it is not our purpose to examine in the present place.

The circumstance of the degrees of natural-historical similarity not being equal, is not merely unprejudicial to its employment, but has rather the effect of rendering it more general. In this manner the natural-historical resemblance becomes the *general principle of classification*; that is to say, it furnishes the means, according to its more distant degrees, of forming representations still more general than those of the genus, should this be of any use in Natural History. Geometrical similarity is absolute, and does not admit of higher or lower degrees. Two triangles are either similar to each other, or they are not similar: we cannot say that two among a number of isosceles triangles, if they have not equal angles, are more similar to each other than to an equilateral or a scalene triangle; or that a four-sided figure is more like a triangle than a pentagon, or a circle. For wherever there do not exist equality of angles, and equal proportions between the sides that are similarly situate, neither can any general similarity exist. The exactness of this idea depends upon the circumstance that geometry takes account, and compares the differences, of only *one* property, extension. Natural History, on the contrary, must reflect upon *all* the physical properties of the objects considered; and this is the reason why the same determinate meaning cannot be attached to it here, which it would have, were we permitted to confine ourselves to single properties. In Geometry no classification could be produced (and it would be superfluous, however) by means of the idea of similarity, because this idea does not include within itself a variety of different degrees; whereas, in Natural History, where a classif [illegible] indispensable, the possibility of arriving at one, which [illegible] nt in all its parts, entirely depends upon the diffe [illegible] atural-historical similarity.

It might be objected to the application of the natural-historical resemblance as a principle of classification, at least in the mineral kingdom, that it does not contain any thing from which we might learn whether a particular individual belongs to one or another species or genus; and certain characteristic terms are then selected or fixed upon, in the representation of the genus or species, by which this purpose could be accomplished. This mode of proceeding, however, becomes the means of introducing inconsistencies and difficulties of various kinds, traces of which we also find in the other departments of Natural History. The reason of this is, that two most essentially different subjects have been confounded with each other, the *original representation*, and the *character* of the species or genera. The first of these consists of the essential unities of the system, and is produced by the application of the idea of species, genus, &c. to nature; the second yields the means of distinguishing these unities, and serves to collect the single individuals found in nature within the compass of these ideas. If both are improperly joined, and employed at the same time, neither of them will be found perfectly to answer the purpose, and we shall be reduced to the necessity of considering bodies in unnatural connections. The natural-historical resemblance, upon which the original representations of the genera, and the higher unities of classification, are grounded, must therefore be confined, as a principle of classification, to these higher ideas, and, as such, is perfectly sufficient; whereas the determination of individuals requires another process, dependent upon different considerations.

This principle of classification is confined to Natural History, but is the same in its three departments. If we intend to classify natural productions in another science than this, we must first have a peculiar principle belonging to the science in question, although the species remains the same; for this, determined according to natural history principles, or corresponding to the natural-historical determination, is the *general object of every classification.* In a chemical classification of minerals, therefore, we must not expect or require that the chemical genera, orders, &c. should correspond to the natural-historical ones; still less should we, in order to avoid or remedy the discrepancies which may arise, employ both principles, the natural-historical and the chemical, at once, or unite them, as has almost universally been the custom in what are generally termed Mineral Systems; for such a practice is in every respect reprehensible, nor has any thing similar to it ever been tolerated in Zoology or Botany.

Having obtained the idea of the genus in Natural History, we may immediately proceed to that of the mineral kingdom, without the intermediate steps of the orders and classes. These, however, are very useful in collecting the individuals within their respective classes, and are produced in the same way as the genera. The Orders, in particular, are very easily recognised in the productions of inorganic nature, and they correspond to the Natural Families of the organic kingdoms. It is to be expected, that greater advantages will yet be obtained from them, for the study of Natural History, when they are more completely known.

The Mineral Kingdom is a series of natural-historical genera, and the Mineral System is its exposition, by means of the systematic unities of classes and orders, which are produced by employing the more distant degrees of natural-historical resemblance. The mineral system is therefore the systematic exhibition of the natural-historical resemblance, as observable in the mineral kingdom, or of the connection established by nature among its products, by means of this resemblance. In this respect it is called the *Natural* system, because in fact it expresses nature in this very remarkable relation. From reasons stated above, this cannot be called the system of Nature, although it seems to approach very near the idea which is connected with that expression by several writers. But it is the only one which deserves the name of a system; for those divisions of the natural-historical productions, which are commonly called artificial systems, ought not to be designated by that name. Though they may be useful in various respects, and applicable also in the mineral system, provided we have already formed a correct idea of the natural-historical species; yet, they do not conduct the exhibition of nature according to the natural-historical similarity explained above, and do not therefore possess any truly natural-historic [illegible]rtance. They would not in this place have been at all a[illegible]re it not for explaining the above mentioned confu[illegible]e artificial systems, the *idea* and the *character* ar[illegible]ame thing, and there is nothing left of those original representations of genera, &c. nor of the natural-historical resemblance, upon which they depend. By the distribution itself, we determine the single characteristic marks which contain those ideas. The reason of the prevailing confusion is, that the classification, or the production of the general idea referring to the natural system, and the division, or the characters of the artificial system, were not sufficiently distinguished, or because it was expected that both of them should be found subservient to the same purposes. In every attempt, therefore, to construct systems, that may answer the purpose for which they are intended in Natural History, we must choose either the one or the other, and carry it through the whole range of our information with perfect consistency, as we should otherwise obtain a mixture of both, which, though it is less objectionable than the union of the natural-historical and chemical principles, in the so-called Systems of Mineralogy, and may even in some respects be useful, yet cannot be regarded as satisfactory in the present scientific state of Natural History.

In regard to the Natural System, we must finally observe, that there can be *only one* of that kind, and that it is impossible *different* natural systems should exist, because there cannot be *different kinds* of natural-historical resemblance. All the attempts toward constructing it, must, however, be acknowledged to be mere approximations to it, the difference of which is grounded in their own imperfection.

The natural system, the only one of which we intend to speak at present, having once been completed, we have next to endeavour to connect its unities with certain words, by which the ideas and representations may be so expressed as to be conveniently applied in writing and speaking, that is to say to construct a *nomenclature*. Nothing is so well calculated to furnish us with an idea of the situation in which Mineralogy has hitherto been placed, as the consideration of what is usually called its Nomenclature, and of the method daily employed in forming new names. Mineralogists seem to be agreed in considering those names the best which have no signification; and if we reckon among these the names derived from colours, persons, localities, and other accidental circumstances, the truth of this opinion cannot be denied. This does not throw a favourable light on the names which have a signification, and which are of two different kinds. Some of them refer to the connection of the different natural productions, in regard to their resemblance, some to their chemical composition. The employment of the latter, which belong to a science entirely different from Natural History, clearly demonstrates, that the science in which they are employed is yet far from being an independent one; and this is perfectly confirmed on farther examination. The connection expressed by the former, is either entirely incorrect, or at least does not refer to the system, in which the names and denominations are applied. They produce erroneous conceptions, and hence are still more objectionable than those that have no signification at all, particularly for beginners, who are not yet accustomed to the examination of minerals themselves. To be convinced of the truth of these observations, we have only to reflect upon the names of blende and hornblende, of cross-stone, and iron-stone, of heavy-spar, schillerspar, adamantine-spar; of white, green, yellow, red, blue, black lead-ore, fahl-ore, cube-ore, red manganese-ore, grey antimony-ore, and many others.

In every science, but particularly in Natural History, it is necessary to give a signification to words, and, therefore, really to express something by them; the question therefore is now, What are the things that should be expressed by the nomenclature in Natural History in general, and more particularly in Mineralogy? There are two objects to be attained in respect to this. The first is to denominate the species, or to determine the object of which something is to be said; the second is to indicate the connection which exists between them, in regard to their natural-historical similarity in the natural system, for this is the ultimate end of all the endeavours of naturalists. Any nomenclature confined to the former of these purposes is a *trivial* nomenclature; it does not presuppose a system, nor any scientific disposition of the species; whereas that in which both are united, and which, therefore, refers to a system, will represent that system, [illegible]lled on that account, being the only scientific one, [illegible] nomenclature.

In those sc[illegible]ve scope to hypothesis we generally prefer [illegible]s (names and denominations), as are free from every thing hypothetical, that they may not be subjected to changes, which are inseparable from such sciences, and hence might become prejudicial or form impediments in their farther development. This does not apply to Natural History; for when pure, that science does not contain any thing hypothetical, hypotheses being only introduced by the intermixture of other sciences. The natural-historical resemblance itself, the only thing which might be objected to, in reference to this subject, is as far from being a hypothesis, as the laws of combination or the connection among the regular forms of a species. The hypothetical denominations of other sciences do not therefore allow any comparison with the systematic denominations of Natural History.

In Mineralogy the systematic Nomenclature has been treated with indifference, or altogether slighted; nor have mineralogists even given themselves the trouble of attempting to compose such a nomenclature. The reason of this is, that mineralogy itself was treated not as a science, but as an aggregate of various kinds of information,—a sort of mixture which would admit every kind of knowledge to be introduced, and in which nothing could be placed wrong, because in such a disposition there could be no order. If we endeavour to give a scientific form to this aggregate, which has been but too generally considered as a science deserving the name of Mineralogy, it becomes necessary to effect a complete transformation of the whole, and also to construct a systematic nomenclature, which becomes indispensable, whenever we leave the path of empiricism, as has been amply demonstrated by experience in Zoology and Botany. The application of a systematic nomenclature, however, is impossible, unless Mineralogy possess a scientific form, and it is for the use of the science as such alone, that it is intended; nay, it would be pedantic to make use of systematic names where science is not the object, and where the names most easily understood are those used in the daily intercourse of life, or by the common miner.

But to the student systematic nomenclature is indispensable, and of the highest utility; because it not only keeps in his mind a vivid picture of the connection existing between the objects named, and thus employs his intellect, but also because it assists his memory to a great extent. Whatever is intended to be regularly taught must be a science; for empiricism does not allow of scientific instruction, but must be acquired like an art, or a handicraft trade, by being shewn its particular processes, or the practical advantages which it admits; and it is a matter of regret that mineralogy should have been so long treated without a scientific form. This is not to be recommended to beginners, for the only method from which they can reap advantage is the scientific one; and as, in the development of every science, we must endeavour, in mineralogy, to consider the facility with which the beginner may be instructed, as one of its principal purposes; and this must be done in a scientific manner, to prepare the way for the more general diffusion of the science. For this purpose, the correctness of the general ideas, and that of the expressions, are equally important. With the above mentioned empirical information, we may, in fact, display a great deal of erudition; but this should not dazzle the beginner, for empiricism only appears the more truly naked, the more it is invested with this ragged covering of learning.

The systematic nomenclature is the most efficient, and we may really say the only means, of confining the arbitrary mode of proceeding in giving names to minerals, and in multiplying them without use or convenience. Those who, by a process to be afterwards explained, have brought an individual unknown to them, within the compass of its Species, will be under no embarrassment for a name to it, but will join it to the name connected with that idea, because this is the more particular object of their proceeding. Though it be admitted that this is sufficient, if the system contains the species to which the individual belongs, it may be asked, Of what advantage will it be, if this be not the case? Still the system may contain the Genus, or the Order, and even then part of the difficulty is already overcome. As examples of this, we shall only mention the hemi-prismatic hal-baryte, and the axotomous lead-baryte, two new species, which have found themselves naturally included in those genera, the nam they now bear. In extreme cases, when an individ does not even belong to one of the orders know becomes expedient to furnish the mineral with its remaining properties being quite indifferent, since it has not yet become an object of the science; and this name may be afterwards replaced by a systematic denomination, which is the only change of names in which we should ever indulge ourselves. To abolish one trivial name, and to introduce another in its stead, does not forward the interest of the science, but merely gratifies personal vanity. As mineralogists are now daily employed in enlarging and perfecting our actual knowledge in the science, such cases must be diminishing in frequency; whereas the difficulties and confusion arising from them would increase, by the endeavours to suppress science and continue empiricism.

The Terminology, the Theory of the system, and the Nomenclature, the three departments of Natural History treated of above, form the constituents of theoretical Mineralogy; practice, or the application of it to nature, requires something more. What must we do, if we have an individual before us, in order to connect the single body in question, the properties of which we have ascertained, with the above-mentioned general ideas, since, though it be contained within them, it presents only a single particular case of the generality considered; and also to provide it with the right name? Or what can we do, to arrive at the knowledge of a mineral, the name of which we know, without having the object itself before our eyes? The solution of both problems depends upon some contrivance of *connecting the general idea with the name*, or of *connecting the name with the general idea*, as produced by the actual examination of the natural productions. And this is more properly the object of Natural History, for which all that has preceded forms but the preparation, or, as it were, the apparatus. This idea of Natural History exactly agrees with the definition of it given by Linnæus, and even with the following passage by Werner. "When I open a work on oryctognosy, it is with the intention either of obtaining a general knowledge of that science; or of acquiring, in particular, the complete conception of a fossil, which I know only by name; or of learning, in respect to a fossil which I have found, and whose external characters I have discovered, what is its name, and what place it occupies in the system of fossils [*]."

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[*] Werner on the External Characters of Minerals, p. 3.

If, according to the same idea, we endeavour to construct the science, we shall obtain only *Natural History, most completely* established, which is the best demonstration of its correctness. The endeavours of naturalists, in all the three natural kingdoms, are directed toward the same point, in so far, at least, as their object is really Natural History, though it should not always be so clearly expressed. For, if we take away from their labours some extraneous additions, which do not regard the essence, but which yet may very often contain information of the highest importance, nothing but pure Natural History remains, and exactly corresponds with the general ideas developed above.

In order to find the denomination, when the properties of the mineral are given, we employ the *characteristic*, which consists of an *assemblage of general ideas*, corresponding to the system, and expressed by *single* distinctive marks. With these ideas are connected the names and denominations, as far as the nomenclature extends and requires, not above the order, nor below the species; and they are by degrees transferred to the individual, in proportion as it is found to enter by degrees within the compass of those general ideas. The single assemblages of distinctive marks, are the *characters* of the classes, orders, genera, and species.

Those who have proceeded consistently throughout the whole science, will not be disposed to introduce properties among those characters which are not natural-historical ones, even though certain advantages might be derived from them for the characteristic, particularly in regard to brevity. These advantages, however, may depend, in a great measure, upon the state of our mineralogical information at the time, as to extent and detail; and may therefore be liable to disappear, whenever our information becomes enlarged. The first law in every science is, that it remain consistent in all its departments; and Natural History being so very simple in its development and application, is, in particular, calculated to derive the greatest benefits from a strict adherence to this principle. The characteristic refers in every instance [illegible] *the individuals*; it yields the means of recognising o[illegible]ng them, as it is commonly called, by the distinctio[illegible]l in the characters of the different classes, orders [illegible] species; it presupposes our having the individuals themselves *before our eyes*, if we wish to arrive at the representation of them. The characteristic is only useful when we have the mineral in our hands; it would, therefore, be an erroneous idea, conducive to nothing but loss of time, were we to study it, in order to obtain some knowledge of the minerals themselves.

The characters are not calculated to produce representations or images of the objects to which they refer; neither those of the individuals which are perfectly determined by single characteristics, nor those of the species, the genus, &c. which do not admit of a similar determination. For this end, we therefore require another contrivance, which forms the fifth and last department of Natural History. It is perfectly correct, that, for an individual, a description, which consists of the indication of all its properties, is quite sufficient; but even this would be of no considerable utility, partly because it would be indispensable to describe every one of the number as individuals of the species, partly also, because, in this case, immediate inspection may be placed instead of the description, to which it is always preferable. The description, properly so called, will, therefore, be applicable only if we intend to convey the idea of some particular individual.

The actual or original representation of the species cannot evidently be produced by the indication of single properties: it cannot be described. For it does not contain any determined characteristic properties, but series of all, which, in these representations, take the place of the single marks, but do not belong more particularly to any of the single objects described. The employment of these series is perfectly illustrated, and rendered evident, by the series of crystallization, which, on that account, obtain a yet higher degree of importance. The species should, therefore, be exhibited in a kind of tabular view, by a general description, in which we consider the species itself as the object, whose characteristic marks are the series in the natural-historical properties. The original representation of the species must necessarily be derived from nature. The object of the general description, is to produce it, without immediately referring to nature; because every person has not the command of so much time, opportunity, and other necessary circumstances, as are required for it. The general description must be arranged in such a manner, as that it may become possible to discover in it the description of every individual contained in the species; so that, in fact, it may be said to include the descriptions of every individual, both known and unknown, without being itself a description, properly so called, at all. The study of the general descriptions is, therefore, to be recommended to all those who wish to acquire a more detailed knowledge of the productions of the mineral kingdom; and we should bestow the greatest possible attention upon the construction and completion of them, in treating the subject of scientific mineralogy.

The general descriptions are independent of systems, and presuppose nothing but the correct idea of the species: we must know what a species is. They are not subservient to the recognition or determination of individuals, because these require single characteristic marks, which must at the same time be well defined, if they are meant to be distinctive; and of such the general description does not contain any thing. This determination is the sole object of the characteristic. Hence we may infer what must be the consequence, if we give the characters such an arrangement, that they may at the same time represent the general descriptions of the species; and the latter such an arrangement, that they may, in like manner, serve the purpose of characters, as is but too generally the custom in mineralogical works. Neither of them will entirely answer their purpose; and those who wish to become acquainted with minerals, or to acquire some natural-historical knowledge of them, find themselves under the necessity of proceeding upon the old empirical plan, notwithstanding the number of works on mineralogy, which may in other respects contain the most valuable information. They must content themselves with a superficial and broken sort of knowledge, to which they themselves do not attach any security, for they have recourse to chemical analysis for confirmation; whereas the methodical way of proceeding leads to information that is solid, connected, and as complete as possible, and which is not only in itself firm, but also forms the scale of measuring and judging of the results of other sciences, in so far as they refer to the same objects.

The assemblage of all the general descriptions is termed the *Physiography*. From the explanations given above, it will plainly appear, that this word does not mean *mere description*, any more than Crystallography means the mere description of crystalline forms. However important it may be to rectify the general ideas, it seems by no means worth while to manifest any very particular nicety about the etymological signification of words. This much, however, is evident, that Physiography should not be used for Natural History in general, nor Anorganography for the Natural History of the mineral kingdom; because both of them form only an important department of the whole of Natural History, and, therefore, the part should not be confounded with the whole. There is no great danger in this respect with regard to crystallography, because here, though the name signifies only one of the departments of the science, yet the connection with the whole is much more easily seen, and nobody can be led into erroneous or incorrect suppositions; whereas, if we do not, in the general idea of Natural History, distinguish rightly between its various branches, we may very easily confound them together, or bestow too much attention upon some one of them, at the expence of the rest, which, indeed, would render Mineralogy liable to the charge of presenting only a partial view, which has been urged in another signification against the method of Natural History.

No science can have more than one character. The character of Mineralogy consists in its forming part of Natural History. It cannot at the same time form also a part of another science, for instance Chemistry, if that science itself be not a part of Natural History, which, in this case, nobody ever maintained. The only fault of this kind that could be introduced in mineralogy, might consist in the too great importance attached to one of its departments to the prejudice of the rest. But they are all equally important, and none must be wanting, if the science itself be meant to form a whole. The case is different with regard to its application. Those who wish to determine an individual occurring in nature, will find the characteristic the most important department, for none of the rest can be of the least use to them; while those who intend to arrive at a general conception of the species, from knowing its name, or one of the individuals belonging to it, will find their views forwarded only by the physiography; for neither the characteristic, nor any other department of mineralogy, contain any information answering the purpose in view.

If we consider, in general, the demands that may be expected to be fulfilled by any part of Natural History, we find, that, under the circumstances detailed above, mineralogy answers them all perfectly; nay, more, that within its peculiar province none can be imagined, to which it does not correspond. But if the object in question lies beyond the limits of Natural History, then this mode of treatment renders mineralogy utterly unfit to answer the questions proposed. Nobody will ever be able to infer from the mere natural-historical consideration of an individual, any thing in regard to its chemical, geological, or other properties. We may dispense with examining the opinions that have been expressed on the subject; because it will be obvious to all whence they have been derived. Natural History, therefore, has its province exactly determined, and its limits distinctly marked out, within which it serves every purpose, but admits of no application without.

These commendable properties are conferred upon mineralogy, as the natural history of the mineral kingdom, solely by making it entirely correspond to the philosophical idea of a science. It contains merely natural-historical information; that is, such as proceeds from a comparison of natural-historical properties, and all the rest is foreign to it. The development of the whole, in its single departments, is in itself systematical; and what it contains of real systems, the systems of crystallization, and the mineral system itself, really deserve that name; because they are the result of the application of one single idea to the whole compass of a certain kind of information. The science itself forms a whole, being intimately connected in all its departments, and strictly separated from all other sciences, which is a necessary consequence of a systematic mode of treatment. The method employed is so simple, that, on that very account, it is *immutable*; nay, we are entitled to maintain, that other methods, compounded of different principles, from the want of consistency prevailing in their different departments, will finally, also, be reduced to this meth [illegible]

Casting now a g [illegible] eginning of this paper, we may resume, that, so fa [illegible] historical properties extend, so far also goes Natural History, and no farther. It has no historical department, properly so called, because, from the examination of the natural-historical properties alone, we cannot deduce any thing like a history of one or of a number of natural productions, which history must evidently consist of something very distant from what is necessary in the explanation of terminology; that, for instance, the seed of a plant germinates, that the young plant itself grows, that it produces flowers and seeds, grows old, and finally dies. Hence every thing allied to history, every thing that happens to natural productions, their uses, and the injuries they occasion, is foreign to our science, and should be mentioned merely in the shape of historical notices, in order to bring other sciences in connection with it, although the science itself has taken its rise from this foreign ground. This is not, however, its *scientific* rise, for, as a science, it could only prosper when planted upon the ground of the natural-historical properties; it means only the first cause of its coming at all within the researches of man.

It is now easy to determine, in what relation natural history in general, and mineralogy in particular, should be to the other sciences, in so far as they are occupied with the same natural bodies. These sciences form the beginning, in a scientific inquiry into the nature of the production; they determine the object, and without teaching any thing that does not enter within the province of Natural History, and thus give it over to other sciences, each of which, according to its peculiar character, produces a mass of information of a particular kind. Although, in themselves, this information be of the highest importance for science, and for the benefit of mankind, yet they lose much or the whole of their value, if we do not know the objects to which they refer, and which to determine, is neither their object, nor does it enter within the reach of their powers. All this is evident of itself, yet we often hear that chemistry and mineralogy mutually presuppose each other. If we say that chemistry presupposes mineralogy, we do not mean to intimate that this is with a view of grounding its own scientific development upon it, but only to have the object of its inquiry determined, and in so far it is perfectly true. But nothing at all can be meant, by saying that mineralogy presupposes chemistry. For, in order to arrive at the rank of a science, chemistry cannot be of any assistance to it, and the objects are determined by mineralogical inquiry for the science of chemistry, and not inversely, which is likewise the case with all other sciences. The proposition, that two sciences mutually presuppose each other, in its perfect generality, has no meaning whatever; for it is true only if the two sciences coalesce into a single one. It is even true of propositions within the same science. We not unfrequently meet with such opinions on the relation of natural history to other sciences;—the only thing that can be said to their advantage is, that they render all refutation superfluous.

ART. X.—*Account of the Bones of various Animals discovered at Breingues, in the Department Du Lot.* By M. DELPON.

THIS discovery has been mentioned by M. Cuvier in the Analysis of the labours of the Royal Academy of Science during the year 1818. Some of the bones in question have been deposited in the Museum at the Jardin du Roi, and M. Cuvier has taken notice of them in his great work; but we have judged it useful to present an extract of the inedited notice of M. Delpon, because it exhibits several very curious facts, especially the very singular order in which these bones have been found.

In various points of the calcareous portion of Quercy, there are seen remains of a sort of entrenchment, formed of blocks of stone, of more or less considerable dimensions, and which describe straight lines or circular inclosures. The most remarkable of these inclosures occupy the summit of two mountains of the Commun de Breingues, in the Circle (*arrondissement*) of Figeac, of which the one is situate on the right bank of the Selé, and the other on the left. There are observed in the rocks of the right bank several cavities or grottoes, before which some vestiges of buildings are seen,—a circumstance which presents itself in the greater number of the grottoes with which the rocks along the Lot, the Selé, &c. are perforated. Popular traditions have occasioned several diggings to be made in these grottoes, with the view of discovering treasures supposed to be concealed in them. In 1816, the whole population of

Breingues was occupied with those of which the present article is intended to furnish some account. In one, among others, of which the opening was almost concealed by the rocks, the entrance was found choked up with earth. The labourers hastened to clear it out, and on coming to the depth of three feet, they found the bones of a human body, beside which was an iron instrument resembling a fork with two prongs. This circumstance tended to redouble their exertions, and the digging was continued in a perpendicular direction, with the aid of a capstan, to the depth of eighteen metres; but the natural cavity, which had uniformly, until now, shewn a straight direction, here presented three cavities, equally filled up with earth and stones. The workmen first followed that which brought them nearest the first grotto, and were presently arrested by three large stones, placed above one another by the hand of man. After having removed them, they remarked that each of them was of a reddish and earthy colour upon one of its faces, like all those which are at the present day raised from the surface of the ground, and that the opposite face was covered with mosses and byssi;—a circumstance which evidently shewed that these stones had for a long time remained in the open air before they had been removed thus far under ground. It was not doubted that they closed the cavity in which the treasure must have been deposited; but in place of this treasure, they found nothing but a prodigious quantity of bones, some of them mingled with the earth or stones, and *others very carefully placed in narrow fissures of the rock.* Several heads of a species of deer, at the present day unknown, and many other bones, were discovered, without any mixture of earth, in a small cavity, covered over with a rude slab, placed with great care. It ought to be remarked also, that here and there the mass of stones and common soil was interrupted by small quantities of an alluvial earth, composed of clay and sand, similar to that which the river Selé deposits at the present day.

It was not only found that no current of water could have brought it there, but it could not be doubted that those small heaps of alluvial earth had been formed by men, since they were pressed, regularly arranged, and entirely surrounded with small calcareous stones of a very white colour, and which must have been soiled by the water, had it deposited these alluvial matters so regularly. Besides the elevation of this grotto being more than 300 metres above the river, precluded the idea that the waters of the Selé could have reached it.

Hoping that they would be more fortunate in the other branches of the gallery, they gave up working in this; but the others presented nothing but bones placed in the same manner. So great a quantity was taken out, that the whole together would have formed a mass of more than twenty cubic metres. The greater number of such as possessed any extraordinary appearance, were broken by the persons who first got hold of them. Some of the bones were incrusted, and others inclosed in a calcareous breccia, with a crystalline paste. The greater number were so well preserved, that they looked as if the flesh had been recently detached from them; but as soon as they were exposed to the external air, they became scaly and whitish.

Among these bones there were recognised the skull of a rhinoceros, three teeth of the same animal; the head of a species of deer now unknown upon the globe, and of which the horns have some resemblance to those of a young reindeer (see the *Recherches sur les Ossemens Fossiles*, t. iv. p. 89); the fragments of the horn of a large species of deer equally unknown, but allied to the common stag; and, lastly, the humerus of a large ox, and a horse's femur.

M. Delpon concludes his notice with some judicious reflections. He infers, from the existence of these bones of animals foreign to our climate, and which have formerly lived on our soil, that the temperature has diminished since the time when it was sufficiently high to allow these animals to live. In a historical point of view, he inquires for what reason their bones had been deposited with so much care in the cavities where they have been found. He thinks that these grottoes were used by the Druids for performing their ceremonies in them, and supposes the bones in question to be the remains of the sacrifices which they had offered to the gods. We are of opinion, that, whatever uses these caverns may have been applied to, according to the times, the bones which are found in them are of a date much anterior to the Druids, and even to the establishment of the human species in these countries; and that their regular arrangement is a result, either of the superstition of the first inhabitants of the country who discovered them, or of the amusement of herdsmen, or some other cause of this description.—*Bullet. Univers. Nov.* 1825.

ART. XI.—*Observations regarding the Position of the Fossil Megalosaurus and Didelphis or Opossum at Stonesfield.*

THE bones of the Megalosaurus occur at Stonesfield, in strata of an oolitic limestone-slate, which is wrought for roofing houses; and in the same quarries, which abound in organic remains, there have been found several portions of a jaw, which undoubtedly belong to a small insectivorous animal of the order Carnivora, which has been by some referred to the genus Didelphis. There occur in the same strata, bones of birds and reptiles, teeth of fishes, elytra of insects, and vestiges of marine and terrestrial plants. Notwithstanding this association of fossils, hitherto regarded as foreign to the deposits beneath the Chalk Formation, English geologists have been led to think that the Stonesfield slate forms part of the middle oolite system; and it is very remarkable, that at Cuckfield, in Sussex (the only place in which there has hitherto been discovered a great number of fossils similar to those of Stonesfield), the strata which contain them form part of the formation of the iron-sand, inferior to the chalk, which is much newer than the middle oolite deposits. The following, according to Mr Buckland, is a list of the fossils, which are found equally in the limestone-slate of Stonesfield and the iron-sand of Tilgate Forest: Bones of birds; of the Megalosaurus; of the Plesiosaurus; scales, teeth and bones of a crocodile; humerus and ribs of cetacea; scales of tortoises; the same variety of shark's teeth (Glossopetra); spines of balistæ; palates, teeth and scales of various fishes; fossil wood, impressions of ferns and reeds; some fragments converted into charcoal, and some rolled pebbles of quartz.

The almost perfect resemblance which the organic remains of the two localities present, has induced Professor Buckland to say, that the earth was undoubtedly placed under nearly the same circumstances at the different epochs during which the two deposits were formed; for, as he adds, the number and thickness of the oolitic strata interposed between the great oolite formation and that of the iron sand, prevent us from supposing, even for a moment, that the two deposits are identical. M. C. Prevost, who has visited Stonesfield, thinks, that the interposition of numerous and thick beds of oolite, not being directly evident in any place between the strata which contain the same fossils, doubts may be raised regarding the relative position assigned to the limestone schists of Stonesfield, as well as regarding the place which should be occupied in the general series of the strata of the Earth, by those which, in the Forest of Tilgate, contain the same fossils. In both places, the strata, which contain the organic bodies, do not appear clearly covered by those of the formations which are said to be more recent; and there are numerous considerations that might lead us to consider the two deposits as having been formed at a period which would be much newer than that of the oolitic formations; in short, that they are tertiary and not secondary deposits.

Art. XII.—*Observations on the Comet of July* 1825. By Professor Gautier *.

The year 1825 will be memorable in the annals of astronomy for the number of Comets observed in it. During an interval of less than three months, it has presented to view four of these bodies, still so mysterious in their appearances and in their nature, but whose motions, as well as those of the other bodies of our system, appear entirely regulated by the great law of universal gravitation. The most interesting of these appearances, in a theoretical point of view, was that of M. Encke's small comet of short period, whose return was calculated and predicted by that able astronomer for the second time, and which was found again precisely in the place and with the motion which he had assigned to it. But the [redacted]arkable of these comets with respect to the duration of and lustre, the only one which has been visible to nd which has presented a perceptible tail, is that which was discovered toward the middle of July in the constellation of Taurus. It appeared without being expected, as a mere nebulosity, and had at first a motion extremely slow. Its motion was afterwards gradually accelerated; it became visible to the naked eye; became invested with a tail, which gradually enlarged; and, after having shone for some time in our horizon in the constellation of the Whale, disappeared from our view about the middle of October towards the south, in the constellation of the *Apparatus Sculptoris*. The first elements of this comet with which I was acquainted, indicating that it would only pass to its perihelion in the month of December next, and thus leaving the hope of still seeing it again, I was curious to assure myself of these circumstances, by calculating myself the elements of its orbit, and deducing from them the different positions in which it ought to be found, with relation to the sun and the earth. It is the results of this calculation that I present here for the use of those who may be interested in the subject.

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* selle, November 1825.

As I did not possess the means of making sufficiently regular and precise observations of this comet myself, I have taken for the basis of my calculation three observations made by M. Plana, at the Royal Observatory of Turin, and inserted in the third number of the thirteenth volume of the *Correspondance Astronomique* of Baron Zach, namely, those of the 25th August, of the 5th, and of the 25th September. For the determination of the elements of the parabolic orbit of the comet according to these observations, I have made use of the method of M. de Laplace, in the application of which I have profited by the excellent instructions which I have previously had the advantage of receiving from MM. Biot and Bouvard. The elements which I have obtained are intermediate between those of MM. Capocci and Hansen, the only ones with which I was acquainted, and approach nearest the latter.

For the purpose of presenting the subject in a clearer manner, I have traced, on a small scale, in Plate IX. Fig. 5. the orbit of the earth, and the portion of the parabolic orbit of the comet near its passage to the perihelion, designating by the same letters in both the positions of these two stars corresponding to the same instants, those of the comet being indicated in large, and those of the earth in small letters. The plane of the figure is that of the orbit of the comet, which has an inclination of 33° 22′ to the plane of the earth's orbit, or to that of the ecliptic, of which the figure presents the elliptic projection. The sun S occupies the focus of each of these orbits. The point E is that in which the earth is at the vernal equinox. It is that from which the arcs of longitude are counted on the ecliptic, from 0 to 360°, in the direction from E to *o*, or in the order of the signs of the Zodiac. The right line NN′ is the line of the nodes of the orbit of the comet, or the line of intersection of the plane of its orbit with the plane of the ecliptic. The point N is what is named the *descending node*, because it is that through which the comet has passed, when it has descended into the portion of its orbit situated beneath the ecliptic. The point N′ is the *ascending node*, or the point through which the comet passes when it ascends above the ecliptic. The position of the line of the nodes is determined by the angle which it makes on the ecliptic with the line SE. I have found the angle ESN′, or the longitude of the ascending node, to be 215° 36′, which gives 35° 36′ for the acute angle ESN.

The point P of the orbit of the comet, or the vertex of the parabola which it describes, is the point at which it is nearest the sun. This is what is called its *perihelion;* and the instant of its passage through this point, as well as its distance from the sun at this instant, are among the number of the most important elements of its motion.

According to my calculation, the comet ought to attain this point on the 10th December of the present year, about 11 in the morning, or more exactly at 10.456 mean time at Paris, reckoned from midnight. The *perihelion distance* SP ought to be once and a quarter the mean distance of the earth from the sun, or more accurately 1.23273, this latter distance being taken for unity. The mean distance of the earth from the sun, or the half of the greater axis of its elliptical orbit, being, as is well known, about thirty-four millions and a-half leagues of twenty-five to the degree; the perihelion distance of the comet from the sun should consequently be about forty-two millions and a-half of these same leagues.

There still remains to be determined the direction of the line SP, and it has usually been done by finding its longitude upon the orbit of the comet itself. For this purpose, a line SE′ is supposed to be drawn upon the orbit, making with the line of the nodes NN′ an angle equal to that comprehended upon the ecliptic between this latter line and the line SE; and the angle E′SP reckoned in the order of the signs, from 0 to 360° proceeding from E′, is what is called the *longitude of the perihelion*. I have found this angle thus reckoned 318° 34′, which gives 41° 26′ for the acute angle PSE′, which is its complement to 360°.

The direction of the comet's motion being from N toward O, we find that this direction projected upon the ecliptic, and, seen from the sun, is contrary to that of the earth's motion upon its orbit, or to the order of the signs, which is from E towards *o*. This is what is expressed by saying that the heliocentric motion of the comet is *retrograde* *.

After having presented the approximative elements of the orbit of the comet, there remains for me to develope the consequences deducible from them, following it in its progress from the first moment of its appearance, and pointing out its successive distances from the sun and the earth, as well as the geocentric positions which it must have assumed since its disappearance.

At the moment of its discovery, which was made on the 15th July at Lucques by M. Pons †, and, on the 19th at Prague, by M. de Biela, the comet was at D, at a distance from the sun S of about twice and two-fifth times that of the earth, and at a

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[*] It is known that this alternative of direction is peculiar to this kind of stars, while all the planets and satellites whose motion is well ascertained move in the right direction. Of the 129 comets whose orbits are now determined, there are 68 in which the motion is direct, and 61 in which it is retrograde.

[†] M. Carlini seems disposed to think (Corr. Astr. t. 13. p. 291.) that it is the comet of Encke, and not the great comet which M. Pons discovered on the 15th July. My elements, however, give me for that day the same declination as that resulting from M. Pons's estimate, and a right ascension, which differs only a few minutes of a degree from his. However this may be, the two comets must have been, at this period, in very near geocentric positions, and it would be singular if no person had observed both of them at once at this moment. The comet of Encke could only have been then at a distance from the earth, nearly equal to three-fifths of that of the other comet.

distance from the earth *d*, of nearly three times that quantity, or of more than a hundred millions of leagues. It was then at an elevation of about 26° above the equator; and was in the part of its orbit situated above the ecliptic. But it advanced rapidly toward this plane, approaching the descending node N, which it attained on the 23d August, about 11 o'clock in the evening. Its motion in longitude being in the contrary direction to that of the earth, the two stars then tended by this circumstance to approach each other rapidly, although the comet must have appeared to remain nearly in the same position with relation to the earth as is shewn by the figure. After the two bodies had been much approximated, the geocentric motion of the comet must have become more rapid, and its brightness less apparent. Towards the 9th October at noon, the comet was in O, and the earth in *o*; the first being in opposition to the sun in longitude, or situated on the side opposite the sun with relation to the earth, and having already descended, relatively to this latter, about 33° 10′ beneath the ecliptic. It was then that the comet and the earth were nearest one another; and, I find that their distance at this period was not more than 0.615 of that of the earth from the sun, or about twenty-one millions and a quarter of leagues. The tail, at this period, had an apparent length of about 12°, although it was then visible to us only as shortened in a very high degree; at least it would be so, were we to suppose it having a direction contrary to the earth, and directly opposite to the sun, as they ordinarily have. On this supposition we should find, that its real length must have been more than eight millions of leagues. M. Pons remarked at that time in it (Corr. Astr. t. xiii. p. 394.) three very distinct rays at equal distances from one another, and of unequal length, presenting some resemblance to the rays of the comet of 1744, such as they have been described by the astronomer de Loys de Cheseaux of Lausanne, in his treatise on that comet.

After this the comet began to remove from the earth, in consequence of the contrary motion of the two bodies, and it was, in fact, remarke [illegible] ter days of its appearance, that the tail already app [illegible] iant. The comet continuing to descend beneat [illegible] quickly disappeared from our view in conseq [illegible] the 18th October, there could only be seen from Geneva a portion of its tail above the mountain of Saleve, the head and nucleus remaining concealed behind the mountain. It is to the inhabitants of the southern countries that the advantage will probably be reserved of seeing this comet at the period in which, from its being then nearest the sun, its tail must be longest. On the 10th December, at the moment of its passing the perihelion, its heliocentric latitude will be about 32° 25′, its distance from the earth P*p* 1.85, or nearly sixty-four millions of leagues; its southern declination about 42° 31′; and its right ascension 296° 25′; so that it will be then situated in the southern part of the constellation of Sagittarius. Its elongation, or its angular distance from the sun, seen from the earth, which, at this moment, will be 67° 20′, will afterwards tend to diminish rapidly; and, towards the 8th January 1826, the comet will be found at C, in conjunction with the sun, or on the same side with that star, with relation to the earth C, and having the same longitude. The distance from the sun will be then 1.311, and its distance from the earth 2.207, or seventy-six millions of leagues. It south heliocentric latitude will be 32° 2′, and the brightness of the sun will for some time conceal it even from the observers above whose horizon it will pass.

After this period, the figure shews that the comet, although continuing to remove from the sun, must tend anew to approach the earth, from the very circumstance of the opposite direction of their heliocentric motion. But the motion in longitude of the comet beginning to become slower, on account of the diminution in curvature of the portion of its trajectory which it then describes, it will be the earth that must traverse the greater part of the arc of longitude necessary in order to its being again found on the same direction as the comet seen from the sun, and between these two stars. This will be a second opposition on the part of the comet, which will correspond to a point of the ecliptic almost opposite to that of the first, and will take place, according to my calculation, towards the 8th May 1826, the comet being then to be found at O′, and the earth at *o*′. The distance of the comet from the sun will be then 2.449, and that from the earth only 1.453, or about fifty millions of leagues. The south heliocentric latitude of the comet will not be more than 7° 17′, its southern declination will be 28° 40′, and its right ascension

220° 21′; so that it will then be situated at the extremity of the tail of Hydra.

It is in the interval between the conjunction and the second opposition, that the comet must reappear to us. But it is conceived, that its distance will then render it less than it has hitherto been, and it is probable that it will not be at all visible to the naked eye at the period of its reappearance. Its depression beneath the ecliptic, which will be greater seen from the earth than from the sun, on account of the great proximity of the latter, will also for some time form an obstacle to its view in the north of Europe, as may be judged by the following geocentric positions of the comet, resulting from my calculation.

	Right Ascension.	South Declination.	Distance from the Sun.	Distance from the Earth.
1826 1st February,	289 .. 25	39.52	1.474	2.222
1st March,	283 .. 21	40.18	1.730	1.971
1st April,	264 .. 4	41.1	2.052	1.549
20th April,	241 .. 28	37.24	2.258	1.392

After the second opposition, the comet will recede at once from the earth and the sun, approaching still nearer the ecliptic, and I find that it will attain this latter plane, or will pass its ascending node N′, towards the 14th July 1826; its right ascension being 195° 40′, and its south declination 6° 41′, which place it not far from the sword of the Virgin. Its distance from the sun will then be 3.178, and its distance from the earth N′ *n*′ 3.085, or about 106 millions of leagues. This great distance from the sun and the earth renders, as is easily seen, the visibility of the comet at this period doubtful. It is a matter of regret that this is the case, on account of the rigorous determination of the orbit that might be obtained in a case when the comet has been observed in its two nodes *.

I must not omit to remark, in concluding this memoir, that the elements on which it rests, result only from a first approximation †. They are also subject to the conditions of the parabolic hypothesis, which is always followed, for the sake of greater facility, in first calculations of this kind; and it is probable that the whole of the observations of this comet taken together, will permit our assigning it a very elongated elliptical orbit. Lastly, it is possible that its proximity to the earth may have produced some influence upon its elements, from the perturbations that may have resulted from it. M. Plana has had the goodness to promise to send me the observations which he made on the 6th and 17th of October, which may serve to rectify my elements. I have judged it proper, however, not to delay the communication of my first results, hoping that they may serve at least to give an idea to those who are not familiar with the theory of comets, of what may be deduced in an approximative manner by means of three observations only.

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* Mecanique Ce [redacted]

† They represe [redacted] one minute of a degree, M. Biela's observation of the 19t [redacted] ami's of the 29th.

P. S.—There is to be found in the 5th Number of the 13th Volume of the *Correspondance Astronomique*, an ephemeris of the comet by M. Capocci, and of M. Hansen's elliptical elements, according to which this comet would make its revolution about the sun in 382 years.

ART. XIII.—*On the Practical Construction of Achromatic Object-Glasses.* By PETER BARLOW, Esq. F. R. S. Professor in the Royal Military Academy, Woolwich. Communicated by the Author. (Continued from Vol. XIV. p. 18).

WE may now proceed to the calculation of the radii for a compound achromatic object-glass, the indices of refraction, and the dispersive power of the glasses being given.

15. *Detail of the computation for a compound Achromatic Object-Glass.*

It is best to make the calculation, in this case, always for a given compound focal length, and afterwards to alter the curvatures in the direct ratio of the proposed focal length to that assumed. Our assumed compound focal length is always 10 inches.

The example we shall propose is to compute the curvatures of a compound object-glass, made from the two specimens of plate and flint experimented upon, as in the leading part of this paper. The index of the plate being 1.528, of the flint 1.601;

and the ratio of dispersion of the two .683; also the required focal length 46 inches.

To find the proper focal length of the two lenses forming the object-glass, so that they may have to each other the ratio of the dispersive powers, and a compound focal length of 10 *inches.*

Rule.—Subtract the number, expressing the dispersive ratio from unity, and the remainder multiplied by 10 will be the focal length of the plate-lens.

2. Divide the focal length of the plate-lens so found by the dispersive ratio, and the quotient will be the focal length of the flint lens.

Example.—In the case we have proposed the dispersive ratio is .683: therefore,

From 1.0000
Take .683
Remainder .317
Multiply by 10 10
3.17 inches focal length of plate.
.683)3.170(4.64 inches ditto of flint.

To find the first or exterior surface of the plate-lens, and the fourth or anterior surface of the flint-lens, for a compound focal length of 10 *inches.*

We must here have recourse to the table given in the subsequent pages, proceeding as follows: In the first column, containing all dispersive ratios, which ever fall within practical limits, find the particular one in question, as, for example, in our case .683; and in the same line in the second and fifth columns, will be found the proper radii of curvature for the first and fourth surfaces, provided the index of the plate be 1.524, and of the flint 1.585: to which numbers the table is computed. In our case these numbers are 6.7956 and 12.7423. But when the tabular indices, as in this example, are not precisely those of the glass in hand, then the above tabular radii must be corrected as follows:

For the Plate Lens.—Find the difference between the tabular index for the plate and that of the glass in question, and multiply by that difference the number standing in the third column. If the given index exceed the tabular index, put the sign *(plus or minus)*, as found in the table, before the product; but if the tabular index be the greater, then prefix to the product the contrary sign to that given in the table.

Next, take the difference between the given index of the flint and that in the table, and proceed exactly in the same manner, viz. multiply this difference by the number in the fourth column, observing, also, the same rule with regard to the sign of the product.

Then, if the two products have the same sign, add them together; but if different signs, subtract them, and prefix the proper sign; that is, the sign of the products themselves when alike; or that of the greater when they are different.

Lastly, if the resulting sign is plus (+), add the number to which it is prefixed to the tabular radius above found; or if minus (—) subtract it; and the sum or remainder will be the corrected radius for the 1st surface of the plate.

Proceed exactly in the same way with the flint lens, using the 5th, 6th, and 7th columns, and we shall then have the corrected radius for the fourth surface.

These rules are illustrated in the following continuation of the example proposed.

The dispersive ratio of our flint and plate, being, by experiment, ·683, the radii for the refractive indices of the table would be for the first surface = 6·7956 inches, and for the fourth surface = 12·7423 inches.

These have now, therefore, to be corrected for the given indices, viz. plate = 1·528, and flint = 1·601.

The difference between the tabular index of the plate, and that given, is ·004: Hence,

No. in 3d column = + ·414
Multiplied by ·004

Gives + ·01656

Next the difference between the tabular index of the flint, and that given, is ·016: Hence,

No. in 4th column = + 2·45
Multiplied by ·016

Gives + ·03920 cor. pl. index.
Add + ·01656 cor. fl. index.

Sum + ·05576 wh. correction.
Add tab. rad. 6.7956

Corrected radius 6·8514 for the first surface.

Again, No. in 6th column = + 116·14
Multiplied by ·004

Gives + ·46456 cor. pl. index.

And, No. in 7th column = — 71·69
Multiplied by ·016

Gives — 1·14704 cor. fl. index.
+ ·46456

Difference — ·68248 wh. correction.
Tab. rad. 12·7422

12·0597 corrected radius for the 4th surface.

It now only remains to find the 2d and 3d, or the contact surfaces. For this determination we have given for each lens the focal length, the radius of one surface, and the index of refraction, which, therefore, for the double convex or plate lens, fall under rule 6 of article 14, and for the flint lens, which is concavo-convex, under rule 8 of the same article.

For 2d surface plate lens, (See rule 6 page 16.)
Focal length = 3·17, Decimal part of index = ·528 ;
3·17 × ·528 = 1.67376 First product
rad. First surface = 6·85·
First prod × rad. 1st surface = 11.4627 = dividend.
6·85 — 1·67376 = 5·17 = divisor,
5·17) 11·4627) 2·22 = 2d surface.

For the 3d surface concave flint lens, (See Rule 8. art. 14.)
Focal length = 4·64, dec. part of index = ·601 ;
4·64 × ·601 = 2·788 . . = 1st product,
12·06 = given rad.
2·788 × 12·06 = 33·62328 = dividend,
12·06 + 2·788 = 14·848 = divisor,
14·848) 33·62328 (2·26 = rad. 3d surface.

We have thus the four following radii, for the successive surfaces to a compound focal length of 10 inches, viz.

Plate { 1st surface radius 6·85 inches convex,
2d surface radius 2·22 inches convex,
Flint { 3d surface radius 2·26 inches concave,
3d surface radius 12.060 inches convex.

These, it will be observed, are for a compound focus of 10 inches, whereas our example required a 46 inch focus. We must therefore increase these several radii in the proportion of 10 to 46, which gives the following results :

As 10 : 46 :: 6·85 : 31·510 1st surface convex,
10 : 46 :: 2·22 : 10·212 2d convex,
10 : 46 :: 2·26 : 10·396 3d concave,
10 : 46 :: 12·060 : 55·476 4th convex.

If the circumstance of the second contact surface, that is the convex one, being the deeper of the two, should be thought a practical inconvenience, or if it should be thought desirable to work these surfaces on pair tools, it will be very easy to reduce the third surface, so as to make it equal to the second, by making such a corresponding change in the fourth surface, as shall still preserve the same focal length, both for the flint lens singly, and for the compound focus. We have only to consider 2·22 inches as the given radius, 4·64 as the focal length, the index being 1·601, and to find the corresponding fourth surface, by the rule given for that purpose, viz. (Rule 9 art. 14.)

Focal length = 4·64 ; Dec. of index = ·601 ;
4·64 × ·601 = 2·78864 = 1st product,
2·22 = given radius,
2·78864 × 222 = 6·19047 = dividend,
2·788 — 2·22 = ·568 = divisor,
568) 6·19047 (10·896 = rad. 4th surface.

And then,

10 : : 46 : 10·898 : 50·13 inches.

This arrangement would therefore give, for the four surfaces,

$$\left.\begin{array}{ll}\text{1st surface} & = 31.510\\ \text{2d} & = 10{\cdot}212\\ \text{3d} & = 10{\cdot}212\\ \text{4th} & = 50{\cdot}13\end{array}\right\}\text{ compound focus 46 inches.}$$

It should be observed, however, that these changes ought to be made as little as possible, because the tendency of them is to produce inaccuracy or defect of compensation; although they may be introduced without a very sensible error in common cases. It is obvious that we might have taken a mean between the two contact radii, and have adjusted both the first and fourth surface accordingly.

There is also another practical convenience which may sometimes be consulted, and which, within certain limits, leads to no error,—this is, when the workman may have a pair of contact tools, which are nearly such as the calculations require. In this case, instead of altering his tools, he may change all the radii in the proportion which the radii required bears to the tools in question. This will make an inch or two difference in the focal length of the object-glass, which will be of no material consequence.

Suppose, for example, that, in the last case, the workman has a pair of contact tools, which measure exactly ten inches, he has only to say, 10.212 : 10 :: 31.51 : 30.85 = 1st sur.
10.212 : 10 :: 50.13 : 49.09 = 4th sur.
10.212 : 10 :: 46.00 : 45.09 = foc. len.

We have thus the following results:

$$\left.\begin{array}{ll}\text{1st surface} & \text{30.85 inches.}\\ \text{2d} & 10.00\\ \text{3d} & 10.00\\ \text{4th} & 49.09\end{array}\right\}\text{ focal length 45.09 inch.}$$

Such is the na[illegible] the calculation required for determining the radii of cur[illegible] construction of achromatic object glasses, and of [illegible] ve one other example.

Example 2.

It is required to determine the radii of curvature for an object glass of 6 feet focus, to be formed of Newcastle plate, whose index is 1.515, and of Swiss flint, whose index is 1.671, the dispersive ratio being .613.

1.000
.613
———
.387
10
———
3.87 = focal length of plate.
.613)3.870(6.31 = focal length of flint.

Tabular radii for dispersive ratio .613 } 1st surface = 6.7131
4th surface = 14.1052

Tabular index plate 1.524 flint = 1.585
Given index plate 1.515 flint = 1.671
———
— .009 + .086

Correction of 1st surface.

Tab. cor. pl. index = + 6.46 for flint index = + .600
— .009 + .086
———
— .05814 + .0516
+ .05160
———
— .00654 = correction.
6.7131
———
1st surface 6.70666 = corrected radius.

Correction of 4th surface.

Tab. cor. pl. index = + 111.90 flint index = — 58.32
— .009 + .086
———
— 1.00764 34992
— 5.01552 46656
———
— 6.02316 = correction. — 5.01552
14.1052
———
4th surface 8.08204 = corrected radius.

And th

Th

To find the contact surfaces.

Focal length pl. lens $= 3.87$. Dec. part of pl. index $= .515$,
$3.87 \times .515 = 1.993 \ldots =$ first product,
rad. first surface $= 6.706$,
$1.993 \times 6.706 = 13.365058 =$ dividend,
$6.706 - 1.993 = 4.713 =$ divisor,
$4.713)13.365058(2.836 =$ rad. 2d surf.
Focal length flint lens $= 6.31$. Dec. part flint index $= .671$.
$6.31 \times .671 = 4.234 \ldots =$ 1st product,
$8.082 =$ rad. fourth surface,
$4.234 \times 8.082 = 34.219188 =$ dividend,
$8.082 + 4.234 = 12.316 =$ divisor.
$12.316)34.219188(2.778 =$ rad. 3d surface.

Hence for a compound focal length of 10 inches we have the following results:

Plate	1st surf. rad.	6.706 convex,
	2d do.	2.836 convex,
Flint	3d surf. rad.	2.778 concave,
	4th do.	8.082 convex.

Therefore, lastly, for our 72 inch compound focus we have:

$$\left.\begin{array}{l} 10 : 72 :: 6.706 : 48.28 = \text{1st surf.} \\ 10 : 72 :: 2.836 : 20.42 = \text{2d} \\ 10 : 72 :: 2.778 : 20.00 = \text{3d} \\ 10 : 72 :: 8.082 : 58.19 = \text{4th} \end{array}\right\} \text{focal length 72 inches.}$$

The above examples will, it is presumed, be found amply sufficient to enable any practical optician to follow out the operations given in the preceding pages, not only as it relates to the computation of his radii, but also for determining the index of refraction, and the dispersive ratio of his two glasses. They are in general suited to those who are but little acquainted with algebraical formulæ, and we therefore offer no apology to those who are algebraists for the length to which some of the calculations ar [illegible] ns have been carried, because they can shorten th [illegible] It may also be proper to observe, that the follow [illegible] xtended from that given by Mr Herschel on [illegible] ich required more than simple proportion; but it is sufficiently accurate for any practical purpose.

TABLE—*Shewing the Radii of the* 1*st and* 4*th Surfaces of Object-Glasses to various dispersive ratios, and to Indices of Refraction* 1.524 *Plate, and* 1.585 *Flint, with Columns of Correction for other Indices.*

Dispersive Ratios.	1st Surface.			4th Surface.		
	Radius to Indices 1·524 1·585	Correction, Plate Index.	Correction, Flint Index.	Radius to Indices 1·524 1·585	Correction, Plate Index.	Correction, Flint Index.
·550	6·7185	+ 7·40	— ·110	14·5353	+ 100·80	— 50·33
·551	6·7182	+ 7·39	— ·100	14·5303	+ 100·99	— 50·45
·552	6·7179	+ 7·37	— ·090	14·5253	+ 101·18	— 50·58
·553	6·7176	+ 7·36	— ·080	14·5203	+ 101·37	— 50·70
·554	6·7173	+ 7·34	— ·071	14·5153	+ 101·57	— 50.83
·555	6·7170	+ 7·33	— ·062	14·5103	+ 101·77	— 50·95
·556	6·7167	+ 7·31	— ·052	14·5053	+ 101·96	— 51·08
·557	6·7164	+ 7·30	— ·042	14·5003	+ 102·15	— 51·21
·558	6·7161	+ 7·28	— ·032	14·4953	+ 102·34	— 51·33
·559	6·7158	+ 7·27	— ·023	14·4905	+ 102·54	— 51·45
·560	6·7155	+ 7·25	— ·014	14·4857	+ 102·74	— 51·58
·561	6·7152	+ 7·24	— ·004	14·4809	+ 102·93	— 51·70
·562	6·7149	+ 7·22	+ ·006	14·4761	+ 103·12	— 51·83
·563	6·7146	+ 7·21	+ ·016	14·4713	+ 103·31	— 51·95
·564	6·7143	+ 7·19	+ ·025	14·4665	+ 103·51	— 52·08
·565	6·7140	+ 7·18	+ ·034	14·4617	+ 103·71	— 52·20
·566	6·7137	+ 7·16	+ ·044	14·4569	+ 103·90	— 52·33
·567	6·7135	+ 7·15	+ ·054	14·4521	+ 104·09	— 52·45
·568	6·7133	+ 7·13	+ ·064	14·4473	+ 104·28	— 52·58
·569	6·7131	+ 7·12	+ ·073	14·4425	+ 104·48	— 52·70
·570	6·7129	+ 7·10	+ ·082	14·4377	+ 104·68	— 52·83
·571	6·7127	+ 7·09	+ ·092	14·4329	+ 104·87	— 52·95
·572	6·7125	+ 7·07	+ ·102	14·4281	+ 105·06	— 53·08
·573	6·7123	+ 7·06	+ ·112	14·4233	+ 105·25	— 53·20
·574	6·7121	+ 7·04	+ ·121	14·4185	+ 105·44	— 53.33
·575	6·7119	+ 7·03	+ ·130	14·4137	+ 105·64	— 53·45
·576	6·7117	+ 7·01	+ ·140	14·4089	+ 105·84	— 53·58
·577	6.7115	+ 7·00	+ ·150	14·4041	+ 106·03	— 53·70
·578	6.7113	+ 6·98	+ ·160	14·3993	+ 106·22	— 53·83
·579	6·7111	+ 6·97	+ ·169	14·3945	+ 106·41	— 53·95
·580	6·7109	+ 6·96	+ ·178	14·3897	+ 106·61	— 54·08
·581	6·7107	+ 6·95	+ ·188	14·3849	+ 106·81	— 54·20
·582	6·7105	+ 6·94	+ ·198	14·3701	+ 107.00	— 54·33
·583	6·7103	+ 6·93	+ ·208	14·3753	+ 107.19	— 54·45
·584	6·7101	+ 6·92	+ ·217	14·3705	+ 107·38	— 54·58
·585	6·7099	+ 6·91	+ ·226	14·3657	+ 107·58	— 54·70
·586	6·7097	+ 6·90	+ ·236	14·3609	+ 107·78	— 54·83
·587	6·7095	+ 6.89	+ ·246	14·3561	+ 107·97	— 54·95
·588	6·7093	+ 6·88	+ ·256	14·3513	+ 108·16	— 55·08
·589	6·7091	+ 6·87	+ ·265	14·3465	+ 108·35	— 55·20

TABLE—*Continued.*

Dipersive Ratios.	1st Surface.			4th Surface.		
	Radius to Indices 1·524 1·585	Correction, Plate Index.	Correction, Flint Index.	Radius to Indices 1·524 1·585	Correction, Plate Index.	Correction, Flint Index.
·590	6·7089	+ 6·86	+ ·274	14·3417	+ 108·54	— 55·33
·591	6·7087	+ 6·85	+ ·284	14·3369	+ 108·74	— 55·45
·592	6·7085	+ 6·84	+ ·294	14·3321	+ 108·94	— 55·53
·593	6·7083	+ 6·83	+ ·304	14·3273	+ 109·13	— 55·70
·594	6·7081	+ 6·82	+ ·313	14·3225	+ 109·32	— 55·83
·595	6·7089	+ 6·81	+ ·322	14·3177	+ 109·51	— 55·95
·596	6·7079	+ 6·80	+ ·332	14·3129	+ 109·71	— 56·08
·597	6·7076	+ 6·79	+ ·342	14·3081	+ 109·90	— 56·20
·598	6·7075	+ 6·78	+ ·352	14·3033	+ 110·09	— 56·33
·599	6·7073	+ 6·77	+ ·361	14·2985	+ 110·29	— 56·46
·600	6·7071	+ 6·76	+ ·370	14·2937	+ 110·49	— 56·59
·601	6·7069	+ 6·73	+ ·388	14·2792	+ 110·60	— 56·72
·602	6·7073	+ 6·71	+ ·406	14·2647	+ 110·71	— 56·85
·603	6·7077	+ 6·69	+ ·424	14·2502	+ 110·83	— 56·99
·604	6·7086	+ 6·67	+ ·442	14·2357	+ 110·94	— 57·12
·605	6·7091	+ 6·64	+ ·460	14·2212	+ 111·05	— 57·25
·606	6·7096	+ 6·62	+ ·478	14·2067	+ 111·17	— 57·39
·607	6·7101	+ 6·60	+ ·495	14·1922	+ 111·28	— 57·52
·608	6·7106	+ 6·58	+ ·512	14·1777	+ 111·39	— 57·65
·609	6·7111	+ 6·55	+ ·529	14·1632	+ 111·51	— 57·79
·610	6·7116	+ 6·53	+ ·546	14·1487	+ 111·62	— 57·92
·611	6·7121	+ 6.51	+ ·564	14·1342	+ 111·73	— 58·05
·612	6·7126	+ 6·49	+ ·582	14·1197	+ 111·85	— 58·19
·613	6·7131	+ 6·46	+ ·600	14·1052	+ 111·96	— 58·32
·614	6·7136	+ 6·44	+ ·618	14·0907	+ 112·07	— 58·45
·615	6·7141	+ 6·42	+ ·636	14·0762	+ 112·19	— 58·59
·616	6·7146	+ 6·40	+ ·654	14·0617	+ 112·30	— 58·72
·617	6·7151	+ 6·37	+ ·671	14·0472	+ 112·41	— 58·85
·618	6·7156	+ 6·35	+ ·688	14·0327	+ 112·53	— 58.99
·619	6·7161	+ 6·33	+ ·705	14·0182	+ 112·64	— 59·12
·620	6·7166	+ 6·31	+ ·722	14·0037	+ 112·75	— 59·25
·621	6·7171	+ 6·28	+ ·740	13·9892	+ 112·87	— 59·39
·822	6·7176	+ 6·26	+ ·758	13·9747	+ 112·98	— 59·52
·623	6·7181	+ 6·24	+ ·776	13·9602	+ 113·09	— 59·65
·624	6·7186	+ 6·22	+ ·794	13·9457	+ 113·21	— 59·79
·625	6·7191	+ 6·19	+ ·812	13·9312	+ 113·22	— 59·92
·626	6.7196	+ 6·17	+ ·830	13·9167	+ 113·43	— 60·05
·627	6·7201	+ 6·15	+ ·847	13·9022	+ 113·55	— 60·19
·628	6·7206	+ 6·13	+ ·864	19·8877	+ 113·66	— 60·32
·629	6·7211	+ 6·10	+ ·881	13·8733	+ 113·77	— 60·45
·630	6·7216	+ 6·08	+ ·898	13·8589	+ 113·89	— 60·59
·631	6·7221	+ 6·06	+ ·916	13·8445	+ 114·00	— 60·72
·632	6·7226	+ 6·04	+ ·934	13·8301	+ 114·11	— 60·85
·633	6·7231	+ 6·01	+ ·952	13·8157	+ 114·23	— 60·99
·634	6·7236	+ 5·99	+ ·970	13·8013	+ 114·34	— 61·12
·635	6·7241	+ 5·97	+ ·988	13·7869	+ 114·45	— 61·25
·636	6·7246	+ 5·95	+ 1·006	13·7725	+ 114·57	— 61·39
·637	6·7251	[illegible]	+ 1·023	13·7581	+ 114·68	— 61·52
·638	6·7256	[illegible]	+ 1·040	13·7437	+ 114·79	— 61·65
·639	6·7261	[illegible]	957	13·7393	+ 114·91	— 61·79
·640	6·7266	[illegible]	74	13·7249	+ 115·02	— 61·92
.641	6·7271	[illegible]	[illegible]	13·7105	+ 115·13	— 62·05
·642	6·7276	[illegible]	[illegible]	13·6961	+ 115·25	— 62·19
·643	6·7281	[illegible]	[illegible]	13·6817	+ 115·36	— 62·32
·644	6·7286	[illegible]	[illegible]	13·6673	+ 115·47	— 62·45

TABLE—*Continued.*

Dispersive Ratios.	1st Surface.			4th Surface.		
	Radius to Indices 1·524 1·585	Correction, Plate Index.	Correction, Flint Index.	Radius to Indices 1·524 1·585	Correction, Plate Index.	Correction, Flint Index.
·645	6·7291	+ 5·74	+ 1·164	13·6429	+ 115·58	— 62·58
·646	6·7296	+ 5·71	+ 1·182	13·6285	+ 115·69	— 62·71
·647	6·7301	+ 5·69	+ 1·199	13·6141	+ 115·70	— 62·84
·648	6·7306	+ 5·67	+ 1·216	13·5997	+ 115.81	— 62·97
·649	6·7311	+ 5·65	+ 1·223	13·5853	+ 116·02	— 63·10
·650	6·7316	+ 5·63	+ 1·25	13·5709	+ 116·14	— 63·23
·651	6·7336	+ 5·58	+ 1·29	13·5457	+ 116·14	— 63·47
·652	6·7356	+ 5·53	+ 1·32	13·5205	+ 116·14	— 63·71
·653	6·7376	+ 5·48	+ 1·36	13·4953	+ 116·14	— 63·95
·654	6·7396	+ 5·44	+ 1·39	13·4701	+ 116·14	— 64·19
·655	6·7416	+ 5·39	+ 1·43	13·4449	+ 116·14	— 64·44
·656	6·7436	+ 5·35	+ 1·46	13·4197	+ 116·14	— 64·69
·657	6·7456	+ 5·30	+ 1·50	13·3945	+ 116·14	— 64·94
·658	6·7476	+ 5·26	+ 1·53	13·3693	+ 116·14	— 65·19
·659	6·7496	+ 5·21	+ 1·57	13·3441	+ 116·14	— 65·44
·660	6·7516	+ 5·17	+ 1·60	13·3189	+ 116·14	— 65·69
·661	6·7536	+ 5·12	+ 1·64	13·2937	+ 116·14	— 65·94
·662	6·7556	+ 5·08	+ 1·68	13·2685	+ 116·14	— 66·19
·663	6·7576	+ 5·03	+ 1·71	13·2433	+ 116·14	— 66·44
·664	6·7595	+ 4·99	+ 1·74	13·2185	+ 116·14	— 66·69
·665	6·7614	+ 4·95	+ 1·78	13.1912	+ 116·14	— 66·94
·666	6·7633	+ 4·90	+ 1·81	13·1683	+ 116·14	— 67·19
·667	6·7652	+ 4·86	+ 1·85	13·1433	+ 116·14	— 67·44
·668	6·7671	+ 4·81	+ 1·89	13·1183	+ 116·14	— 67·69
·669	6·7690	+ 4·77	+ 1·92	13·0933	+ 116·14	— 67·94
·670	6·7709	+ 4·72	+ 1·96	13·0683	+ 116·14	— 68·19
·671	6·7728	+ 4·68	+ 1·99	13·0433	+ 116·14	— 68·44
·672	6·7747	+ 4·63	+ 2·03	13·0183	+ 116·14	— 68·69
·673	6·7766	+ 4·59	+ 2·06	12·9933	+ 116·14	— 68·94
·674	6·7785	+ 4·54	+ 2·09	12·9683	+ 116·14	— 69·19
·675	6·7804	+ 4·60	+ 2·13	12·9431	+ 116·14	— 69·44
·676	6·7823	+ 4·45	+ 2·17	12·9179	+ 116·14	— 69·69
·677	6·7842	+ 4.41	+ 2·21	12·8928	+ 116·14	— 69·94
·678	6·7861	+ 4·36	+ 2·25	12·8677	+ 116·14	— 70·19
·679	6·7880	+ 4·32	+ 2·29	12·8426	+ 116·14	— 70·44
·680	6·7899	+ 4·27	+ 2·33	12·8175	+ 116·14	— 70·69
·681	6·7918	+ 4·23	+ 2·37	12·7924	+ 116·14	— 70·94
·682	6·7937	+ 4·18	+ 2·41	12·7673	+ 116·14	— 71·19
·683	6·7956	+ 4·14	+ 2·45	12·7423	+ 116·14	— 71·44
·684	6·7975	+ 4·09	+ 2·49	12·7171	+ 116·14	— 71·69
·685	6·7994	+ 4·05	+ 2·53	12·6920	+ 116·14	— 71·94
·686	6·8013	+ 4·00	+ 2·57	12·6669	+ 116·14	— 72·19
·687	6·8032	+ 3·96	+ 2·61	12·6418	+ 116·14	— 72·44
·688	6·8051	+ 3·91	+ 2·65	12·6167	+ 116·14	— 72·69
·689	6·8070	+ 3·87	+ 2·70	12·5916	+ 116·14	— 72·94
·690	6·8089	+ 3·82	+ 2·74	12 5665	+ 116·14	— 73·19
·691	6·8108	+ 3·78	+ 2·78	12·5414	+ 116·14	— 73·44
·692	6·8127	+ 3·73	+ 2 82	12·5163	+ 116·14	— 73·69
·693	6·8146	+ 3·69	+ 2·86	12·4912	+ 116·14	— 73·94
·694	6·8165	+ 3·64	+ 2·90	12·4661	+ 116·14	— 74·19
·695	6·8184	+ 3·60	+ 2·94	12·4410	+ 116·14	— 74·44
·696	6·8203	+ 3·55	+ 2·98	12·4159	+ 116·14	— 74·69
·697	6·8222	+ 3·51	+ 3·02	12·3908	+ 116·14	— 74·94
·698	6·8241	+ 3·46	+ 3·06	12·3657	+ 116·14	— 75·19
·699	6·8260	+ 3·41	+ 3·09	12·3406	+ 116·14	— 75·44
·700	6·8279	+ 3·35	+ 3·12	12·3154	+ 116·14	— 75·70

ART. XIV.—*Notices regarding the Vineyards of Egypt.*

A NEWLY published edition of Horace, has given rise to a recent discussion regarding the wines of Egypt. An anonymous writer in one of the journals, does not admit that the *Vinum mareoticum*, mentioned in the 37th ode of the 1st book, came from the neighbourhood of the lake Mareotis in Egypt, but rather from a district of Epirus, which was named *Mareotis*. M. Malté Brun contradicts this opinion; and gives a critical examination of the two passages in which Herodotus says, *1st*, That there are no vines in Egypt; and, *2dly*, That the people drank beer; but that the priests received an allowance of wine daily. He adds, that M. Champollion the younger has recognised upon Egyptian monuments, offerings made to the gods, of two white flagons, which are painted red up to the lower part of the neck, indicating a liquor of that colour; and the Egyptian word *erp*, which signifies *wine*, written beside the flagons, removes all uncertainty with regard to the materials of the offering. Strabo saw wines in Egypt in the neighbourhood of Alexandria, which he mentions as the soil in which the *mareotic wine* was produced. He also saw vines in other districts in Egypt, and he correctly distinguishes their various qualities. Pliny and Athenæus speak not less pertinently of them. Horace must therefore have meant, by *Vinum mareöticum*, the wine of the territory of Mareotis, near Alexandria in Egypt. Lucan even goes so far as to make an important critical distinction, for he warns against confounding the *Mareotic* wine with the exquisite wine which came from *Meröe*. There can remain no doubt regarding the consequences of this letter of M. Malté Brun, namely, that, under the Greek and Roman kings, Egypt had vines, and made wines of various qualities; but, before the Greek kings, was it equally so; and does Herodotus, who at that period travelled in Egypt, speak truly, when he says, that there were none? The following note from one of the editors of the Bulletin des Sciences, goes to solve this interesting difficulty:

"The readers of the *Journal des Debats* have seen with interest the animated discussion which has arisen upon the subject of the Mareotic wine of Egypt. M. Malté Brun has clearly proved the existence of wine in Ancient Egypt, and the weakness of the arguments which have been adduced in opposition to this fact. He might have added a decisive argument, the paintings of the ancient hypogées of the Thebais, among which there have been discovered, twenty years ago, representations of the vintage, and of the manufacture of wine in all its stages, as well as transparent vessels, through which the wine contained in them is seen, so as to leave no doubt remaining with regard to the use of that substance among the Egyptians *. There have been found also among the ruins of the cities, broken amphoræ, and at their bottom the very residue of the wine, in which the tartar was preserved. These facts, taken in connection with the passage in Herodotus, where four arysteres of wines are allotted to each of the two thousand guards of the king daily, effectually remove all uncertainty with regard to the vineyards of Egypt. Nor is M. Costay, in his interesting memoir upon the grottoes of Elethyia, difficulted by the other passage of Herodotus regarding the use of beer in Egypt †; he does not even think it necessary to combat the consequences which have been drawn from it ‡.

It is thus that the attentive traveller may dispel, by a single observation, the mists which the most profound erudition cannot always dissipate, especially when authors contradict each other, and when the same writer plainly contradicts himself, as is the case with Herodotus in the matter referred to above. However, independently of the discovery of the French travellers, it might perhaps have been observed, that the historian who denies the use of wine to the Egyptians, in the 77th chapter of his second book, accords a portion of grape wine to the Egyptain priests in the 37th chapter, and four measures of wine to the warriors in the 168th chapter, which shews that he had at first interpreted, in a certain sense, what he had been inform-

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* The same fact has been observed in the paintings of Thebes.—*Description des Hypogées*, chap. ix. p. 335. 1st Edition; and vol. iii. p. 63. 2d edition, as well as plate xlv. of vol. 2. of the Atlas.

† "As they have no vines in their country, they drink beer."

‡ Descript. de l'Egypte, aut. mem. t. i. p. 61. 1st edition; and t. vi. p. 112. 2d edition; as well as Plate 68 of vol. 1. of the Atlas.

ed with regard to the use of beer,—that, in fact, he had concluded from it, that there were neither vines nor wine in Egypt,—and that, at a later period, when better informed, he had given up this opinion, but had neglected to efface it.

With regard to the nature of the soil and climate of Egypt, there certainly is nothing in them that could induce us to think the vine should not thrive there, or that wine could not be made. The chemists of the French expedition (it will suffice to name Berthollet), occupied themselves with means for introducing a good method of making wine. They knew the vineyards, and the bad wine of Fidimine, a christian village of the province of Fayoum in Upper Egypt; they knew that the grape was of a much better quality, and far superior to the grape of Alexandria; the same, without doubt, as that from which the famous Mareotic wine was made, and which is well known at our tables; it was ultimately proposed to make wine of it for the use of the army, which could not have been very difficult. I have remarked, that the soil was somewhat sandy in the vicinity of Fidimine, and that of the ancient Marea, is of the same nature.

This consideration completes the removal of all uncertainties. Vines were not planted in the muddy soil, in Egypt properly so called, as has already been remarked by learned men; but upon the border of the desert, a little above the level of the inundation. This cultivation was not limited to the Mareotic district, nor to that of Arsinoe, since the same quality of soil occurs every where upon the confines of the valley of the Nile; since at Elethyia, in a very insignificant catacomb, they have amused themselves with painting the gathering of the grapes, and the making of wine, in imitation, no doubt, of what was taking place in the neighbourhood. Thus, without speaking of the wines of Anthylla and Coptos, we have vineyards in Egypt under the 31st parallel, under the 29th and 25th, and from which wine might be provided for the annual consumption of Pharaoh's guards, (about 730,000 pints), besides the wine consumed by the priests. Could we still doubt the existence of vines in Ancient Egypt, it would suffice to read the following passage in the Book of Numbers, chap. xx. ver. 5.: "And wherefore have ye made us to come up out of Egypt, to bring us in unto this evil place? it is no place of seed, or of figs, or of vines, or of pomegranates." Egypt was so far from being destitute of the vine, that a very ancient author goes so far as to say, that the vine was discovered near Plinthine; and, even according to Diodorus Siculus, it was Osiris, the Egyptian Bacchus, that discovered the vine at Nysa, and instructed men in the art of extracting wine from it. If Nysa be placed in Arabia, it is, without doubt, because there is meant the country which separates the Nile from the Arabian Gulf, a vast tract, often called *Arabia* by authors, and full of valleys adapted for the cultivation of the vine.

With regard to the wine of *Meröe*, which appears attested by grave authorities, its existence is, without doubt, more authentic than that of the wonderful wine of Ethiopia, which astonished Semiramis, and not without reason, for it filled, as is said, a lake of 160 feet in circumference; and whoever drank of it was immediately brought to the recollection of his faults, even those which had long been forgotten. Whatever, further, may be the quality of the wines of Egypt, or of those of Ethiopia, we are strongly disposed to conclude, with M. Malté Brun, that those of France are in no way inferior to them.

ART. XV.—*Account of a newly invented and rotatory Gas-Burner.* By Mr JAMES NIMMO, Edinburgh.

As you have occupied many of the pages of your useful Journal lately with discussions respecting the illuminating powers of coal and oil gas, and the best contrivances which have been made for burners of it, allow me to lay before your readers a description of one which I invented some months ago, and which I think is capable of many useful applications.

This burner is no less remarkable for the unexpected effect which it exhibits, than for the real simplicity of its construction. Its peculiarity is, that it has an incessant rotatory motion, which, when combined with a tasteful variation of the burning jets, produces an agreeable and beautiful effect. The following is a description of it: The revolving burner consists of an outside case or tube A (Pl. IX. Fig. 6.), which is filled with water three parts full; and B is a tube which rises from the bottom, and through the centre of the water-case, and which is perforated with holes above the top of the water-case. In the top of the tube B is a steel centre, terminating with a fine point, upon which the inverted tube C revolves. This inverted tube C seals itself in the water, and does not allow the gas emitted from the holes of the tube B to escape; and it has four arms of equal length, and finely bored with small holes at the extremity of each arm for the gas to burn at. All these holes are and must be at the same side of each arm, to give the burner motion; some of the holes are put in a vertical direction, and some inclined at angles to those holes in the sides. This part of the burner is susceptible of great variety of contrivances, and may be carried into multiform shapes and figures, which, added to the perpetual revolving motion of the whole, gives a beautiful brilliancy, very pleasing to the eye.

The theory of it is extremely simple, and is only, *that* the rotatory motion is produced by the pressure of the gas from the *gasometer* being so diminished on one side of the arms of the burner, by the small emission gas-holes, as to cause an increase of pressure on the other side of the tube, and thus to make the whole revolve by the smallest pressure the gas can burn at. The water-joint is necessary to prevent the escape of the gas, and to allow the burner to have an easy motion on the steel point. The *revolving burner* is very plain and simple in its action; but I am convinced, from the many attempts that have been unsuccessfully made by others to discover such a contrivance, that, were it publicly known, it would be of great use and convenience for many of the purposes of life.

ART. XVI.—*Notice regarding the Phosphate of Lime of the Coal Formation.* By M. P. BERTHIER.

TOWARD the end of the last year, Messrs Manby and Wilson sent to [illegible]atory of the School of Mines, for examination, specime[illegible] rent ores of iron which the Riant Company prop[illegible] mong those specimens there was *one* which cont[illegible] ttle iron, and which I presently perceived to be chiefly composed of phosphate of lime. This specimen had absolutely the same appearance as the argillaceous carbonate of iron, and the ticket attached to it indicated that it was found under the same circumstances, that is to say, in kidneys, in the bituminous shales that accompany the coal. It was lenticular, of the size of the fist, homogeneous, very fine granular, having some lustre in a very strong light, and of a deep grey colour. The argillaceous carbonate of iron, of the coal deposit, often contains phosphoric acid, and even in considerable proportion; but until now, the phosphate of lime, in a nearly pure state, has not been observed in this formation. The fact, interesting as it is in a geological point of view, deserves also the notice of metallurgists, and should induce them to institute a strict examination of the ores with which the coal deposits furnish them.

The specimen of the Fins phosphate of lime, on being analysed, yielded the following results:

Lime, - - - -	0.363	Phosphate of Lime (apatite),	0.670
Phosphoric Acid, - -	0.310		
Protoxide of Iron, - -	0.096	Carbonate of Iron, -	0.157
Alumina, - - - -	0.090	Alumina, - - -	0.190
Water, Bitumen, & Carbonic Acid,	0.120	Water and Bitumen, -	0.060
	0.979		0.977

Heated, without addition, in a covered crucible, it melts into a compact, opaque, stony mass, covered at the surface with small, shining metallic grains. Assayed with half its weight of borax, it produces a glassy and enamelled scoria, and very fragile granules, which have scarcely any action upon the magnetic needle.

M. Jules Guillemin, a pupil of St Etienne, attached to the mines of Fins, has addressed to me a note, dated the 31st July, which contains some interesting information relative to the geological position of this ore, and to its ordinary mixtures. I here subjoin an extract from this note.

"This mineral is in nodules of a globular form, sometimes flattened, always of a rather small size. These nodules occur in great quantity in the black argillaceous schists, which separate the second bed of coal from the sandstones that support it; they are not homogeneous; their crust is almost entirely composed of carbonate of iron. Sometimes they contain a great quantity of transparent, laminar, carbonate of lime, which divides the mass into small prisms; sometimes it is coaly matter, and at other times they are enveloped with a crust of compact sulphuret of iron. In the centre is a nucleus of a pale-yellow or grey colour, compact, fine granular, having the appearance of brown flint, and traversed by impressions of gramineæ: it is this nucleus which contains the phosphate of lime. I have found in a specimen, the specific gravity of which was 2.65,

Lime,	0.469	Phosphate of Lime,	0.863
Phosphoric Acid,	0.394		
Protoxide of Iron,	0.072	Carbonate of Iron,	0.117
Carbonic Acid,	0.045		
Alumina,	0.006	Alumina,	0.006
Coal, Water, and loss,	0.014	Coal, Water, and loss,	0.014

"But the relative proportion of phosphate of lime and carbonate of iron varies much. The crust of a nodule assayed in a covered crucible, without addition, gave 0.20 of hard cast-iron (*de fonte dure*), equivalent to 0.43 of carbonate of iron, and a slag weighing 0.56, which was opaque, of an apple-green colour, and entirely similar to melted phosphate of lime."—*Annales des Mines* 1825.

ART. XVII.—*Observations made for Determining the Progress of the Horary Variations of the Barometer under the Tropics, from the Level of the Sea to the Ridge of the Cordillera of the Andes.* By M. DE HUMBOLDT.

M. DE HUMBOLDT, in the volume of his Travels lately published in Paris, states the following interesting conclusions regarding the horary variations of the barometer under the Tropics.

1*st*, The horary variations of the barometer are perceptible in all parts of the earth, and to the height of 2000 toises. They are periodical, and consist of two ascending motions and two descending motions, which are performed in the interval of a day. The periods of the *maxima* and *minima* are not equidistant; they present sepa[illegible] hours. The *maximum* of the morning falls betw[illegible]nd 10½; the *minimum* of the afternoon, between [illegible] the *maximum* of the evening, between 9 hours a[illegible] minimum of the night, between 3 hours and [illegible]

In the equatorial zone, there may be admitted, for these four periods, 21¼, 16, 10½, 16; and, in the temperate zone, 20½, 3¼, 9½, 17; these numbers expressing the hours counted from noon.

2. In the temperate zone, the periods of the *maximum* of the morning, and of the *minimum* of the evening are nearer, by 1 or 2 hours, to the passage of the sun through the meridian in winter than in summer. Observations are wanting regarding the *minimum* of the night. M. de Humboldt recommends them to be made.

3. In the torrid zone, the hours of the *maxima* and *minima* are the same at the level of the sea, and on plains of from 1300 to 1400 toises in height. This is asserted not to be the case in some parts of the temperate zone. On Mount St Bernard, for example, the barometer falls at the same hours at which it is rising at Geneva.

4. Near the *maxima* and *minima*, the barometer is almost stationary during a more or less considerable period; this period varies from 15′ to 2 hours.

5. Between the equator and the parallels of 15° N. and S., the strongest winds, tempests, earthquakes, and the quickest variations of temperature and humidity, do not interrupt or modify the periodicity of the variations. In India, on the contrary, the rainy season entirely disguises the type of the horary variations in the interior of the Continent, on the coasts, and in the straits, although in the open sea they remain unaltered.

6. Between the tropics, a day and a night suffice for knowing the extreme points, and the duration of the variations. In the latitudes of 44° and 48°, they are very distinctly manifested in means of from 15 to 20 days.

7. The extent of the diurnal variations, at the same hours, and in different months, is not the same. This extent also decreases in proportion as the latitude augments.—(See the annexed Table). Lastly, The maximum of the morning is a little higher than the maximum of the evening. The height of the place does not influence these results.

8. The barometrical means of the months differ among themselves from $\overset{\text{mm}}{1.2}$ to $\overset{\text{mm}}{1.5}$, between the tropics; and from 7 to 8 millim. near the tropics, nearly as in the temperate zone. The

extreme annual variations are at the same hours, near the equator, from 4 to 4½ millim.; near the tropic of Capricorn, 21 millim.; near the tropic of Cancer, from 25 to 30 millim.

9. Under the tropics, as in the temperate zone, on comparing the extreme variations of the barometer month by month, the limits of the ascending oscillations are found two or three times nearer than the limits of the descending oscillations.

10. The observations which have been hitherto collected have not indicated a sensible influence of the moon upon the oscillations of the atmosphere; these oscillations appear owing to the sun, which acts, not by the attraction of its mass, but as a calorifying planet. If the solar rays produce periodical changes in the atmosphere, there remains to be explained, why the two barometrical minima nearly coincide with the warmest and coldest periods of the day and night.

Table of Observations of Horary Variation made between the parallels of Lat. 25° S., and Lat. 55° N. from the level of the Ocean to 1400 toises of elevation.

TORRID ZONE.

PLACES OF OBSERVATION.	Minima of the Night.	Maxima of the Morning.	Minima of the Day.	Maxima of the Evening.	Mean extent of Oscillations in 100ths of a millim.	OBSERVERS.
Equatorial Atlantic Ocean,	4h	10h	4h	10h	...	Lamanon & Monges.
Equatorial America, between Lat. 23° N. and 12° S. to 1500 toises of height, - -	4½	9¼	4½	11	2.55	Humbolt & Bonpland.
Payta (Peru), Lat. 5° 6′ S.	3	9	3½	11¼	3.40	Duperrey.
Guayra, Lat. 10° 36′ N.	...	9½	3½	10	2.44	Boussingault & Rivero.
Bogota, Lat. 4° 35′ N. Height 1366 toises.	4	9	4	10	2.29	
Indian and African Seas, Lat. 10° N. 25° S.	4	8½	4	11	...	Horsburgh.
Equatorial Pacific Ocean,	3½	9¼	4	10½	...	Langsdorff & Horner.
Sierra Leone, Lat. 8° 30′ N.	5	9½	3¾	10	...	Sabine.
Mysore, Lat. 14° 11′ N. height 400 toises. — (Rainy Season), -	5	10½	4	10½	...	Kater.
Pacific Ocean, between Lat. 24° 30′ N. and 25° S.	3½	9½	3½	9¾	...	Simonoff.
Macao, Lat. 22° 12′ N.	5	9	5	10	...	Richelet.
Calcutta, Lat. 22° 34′ N.	6	9¼	6	10	...	Balfour.
Equinoctial Brazil, at Rio Janeiro, (Lat. 22° 54′ S.) and at the missions of the Coroatos Indians,	3	9¼	4	11	2.34	Dorta, Freycinet, Eschwege.

paper was instantly reddened, nearly as powerfully as if lime had been employed.

This effect was considered as accidental, and as probably arising from some adventitious alkaline matter, or soap, adhering to the vessels employed. Another experiment was made, with greater care, in an agate-mortar, but with the same, or even a more decided result, in consequence of the more minute division of the material. When pulverized on perfectly clean and polished surfaces of iron, steel, zinc, copper, silver, and platinum, the effect took place, and apparently with equal facility; but it was found that the presence of small quantities of oxide of iron greatly diminished it, in consequence, as was afterwards proved, of the particles of glass being by them defended from the contact of water.

Since there are some saline bodies and metallic combinations which give indications of alkali to turmeric paper, although perfectly neutral compounds, and as pure magnesia reddens this paper when moistened with water, although no solution can be shewn to take place, possibly this might be an effect of the kind, it scarcely appearing probable that any soluble matter should be abstracted from the powdered glass by the mere affusion of pure water. Litmus paper, therefore, reddened by an acid, and paper stained with the blue infusion of cabbage, were also employed as tests; the former had its blue colour restored, and the latter was rendered green.

A portion of flint-glass, in fine powder, was boiled in water for some hours; upon being allowed to cool and subside, the clear portion was decanted and evaporated, and became strongly alkaline to the taste, and to other usual tests; a drop of its concentrated solution, gradually evaporated on a glass-plate, on exposure to the atmosphere, in a short time became deliquescent. Tartaric acid produced an effervescence, and afterwards a precipitate in this solution; as likewise did muriate of platinum. From these experiments, therefore, it may be fairly inferred, that the alkali removed from the glass was potash in an uncombined state, and that the alkaline effect, combined in the first instance, did not depend upon the presence of any alkaline salts, or combination, adhering to or diffused throughout the glass.

The remaining sediment from the above solution, after having been repeatedly washed in successive portions of water, became inert as to its action on test papers, not affecting their colours in the slightest degree; but, upon *trituration*, its alkaline power was again developed; this property being evidently dependent upon the exposure of a new or undecomposed surface. A slight application of heat to the water was found greatly to facilitate this evolution of alkali.

In order to determine the quantity of alkaline matter abstracted from a given weight of glass, by long and continued boiling, 100 grains of flint-glass, in fine powder, were boiled nearly every day for some weeks, in two or three successive portions of water; after this process, the insoluble residue was found deficient in weight by nearly seven grains. This result, however, must not be considered as accurate, but as a mere approximation: for, on the one hand, small portions of glass might have been carried away in the supernatant liquor; and, on the other, more alkali might have been abstracted by repeatedly triturating during the process, which, under these circumstances, would be almost unlimited.

To some pure, dilute, muriatic acid was added very fine flint-glass, in powder, till it was completely neutralised by its alkaline effect. Upon being allowed to subside (which, however, was not very readily effected, minute particles remaining suspended for weeks together), the clear portion afforded a crystalline salt on evaporation, having the characters of muriate of potash.

It may be remarked, that this solution, when *perfectly clear*, contained no lead, on testing for it by sulphuretted hydrogen; but upon agitating or diffusing the fine powder of glass through water, holding the gas in solution, it was immediately discoloured or blackened.

Flint-glass, although chosen for the above experiments, is not the only variety possessing this remarkable property; crown and plate glass, white enamel, and what is more remarkable, Newcastle green-bottle glass, and tube of the same material (in the composition of which there is, comparatively, little alkali), also Reaumur's porcelain, made from the green-bottle glass, possess the power of acting upon vegetable colours as alkalies.

These experiments, tending to prove that glass is a body of irregular composition, parting readily with its alkali by the action of water, it became a matter of some interest to determine how far certain natural combinations of potash with siliceous matter were equally active to the same tests, especially as in green-bottle glass, which contains little alkali, it is thus rendered evident. No analogous effect could, however, be produced by powders of felspar, basalt, greenstone, granite, obsidian, pumice, and some others, even when boiled with water, a method which never failed to produce it rapidly with glass, although cold water is perfectly sufficient.

Some interesting conclusions may be drawn from the above experiments, which may tend to explain several well-known phænomena.

In the first place, with regard to the glasses employed, in the laboratory, or for domestic uses, it must be evident that water has the power of acting upon and dissolving the alkali at the surface, and leaving an insoluble portion spread as a coating over the interior of the vessel, defending it from further immediate action.

Where, however, time can be allowed, the effect does not appear to be confined to mere surface. In collections of ancient glass, specimens may be selected, exhibiting how extensively an analogous action has been going on during the period they have remained buried in the earth. These vitreous relics of antiquity are often covered, to a considerable thickness, with opal pearly scales of beautiful appearance, consisting almost wholly of silica, whose alkali had been removed probably by the action of the water *.

A fragment of transparent ancient glass was examined with regard to its alkaline property, which it was found to enjoy in a high degree, being sensibly alkaline (when in powder) to the tongue, and its hot solution acting upon the cuticle. It appeared to consist almost entirely of potash and silica; not the smallest trace of lead being discoverable in it; several other coloured specimens of ancient glass, upon examination, were, in every

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* The opal is a h [illegible] May not its formation have taken place by a similar agency acti [illegible] binations? The removal of alkali from siliceous compounds [illegible] us constituted.

case, more highly alkaline than any modern glass containing lead, that has hitherto been examined.

The specific gravity of common flint-glass was taken by way of comparison with the ancient fragments above mentioned, the result of which is here given. Flint-glass, S. G., 3.208. Ancient glass, 2.375. It may here be remarked, that the latter acted powerfully upon the test paper, by merely moistening it, without reduction to powder. It cannot be surprising, therefore, that ancient glass, which may almost be called pure silicate of potash, should be occasionally found in states of such rapid decay, as the specimens in collections often exhibit.

Another proof of the action of water, aided by other concomitant circumstances, in producing decomposition upon glass, is an account given in vol. i. p. 135, of the Quarterly Journal of Science, of some bottles of wine, found in a quantity of black mud at the bottom of an old well, full of burned wood, supposed, upon good authority, to be of anterior date to the fire of London (1666). The siliceous earth, in this instance, separated in films on the surface of the bottle, in consequence of the abstraction of alkaline matter, probably by the action of water, aided perhaps originally by a certain degree of heat, and afterwards by the long period of their continuance in a situation favourable to the decomposing agency.

In contact with ammoniacal, or decomposing animal matter, the disintegration of glass takes place more rapidly. Stable-windows, and bottles kept in such situations, often present a very beautiful iridescent appearance, in consequence of the siliceous matter being developed in thin plates on its surface, often amounting to a pearly, and sometimes almost metallic, appearance; an effect which, it is believed, has not been hitherto investigated.

Solution of potash acts very rapidly upon glass, as the chemist, often inconveniently, learns by the effect produced upon the bulb of a thermometer, employed to determine its boiling point, and which is always found corroded to a considerable extent after the experiment.

It may also here be remarked (although not perhaps immediately connected with the subject), that from frequent observations by a person in the habit of using solid carbonate of ammonia, the flint-glass bottles in which it has been for some time kept are invariably rendered much more brittle, and pieces of glass fall out upon very slight motion of its contents. This fact is merely mentioned as curious, and may probably be hereafter more fully examined.

ART. XIX.—*Observations and Experiments on the Structure and Functions of the Sponge.* By ROBERT E. GRANT, M. D., F. R. S. E., F. L. S., M. W. S., &c. (Continued from Vol. XIII. p. 124.)

ALTHOUGH a minute examination of the internal structure of the living sponge is obviously the most natural and necessary step towards discovering its mode of growth and generation, and consequently the place this substance occupies in the scale of beings, and is certainly that most likely to lead to the discovery of some more fixed and scientific principles for discriminating the species, than the vague characters hitherto employed; yet we can scarcely discover, in the writings of zoologists, since the time of Aristotle, any attempt to investigate its structure in a scientific manner. Although Pallas, Lamouroux, Lamarck, Schweigger, and almost every modern zoologist, have considered the examination of this animal, in its recent state, as still an important desideratum in comparative anatomy; yet the deficiency is generally supplied only by ingenious conjectures from the appearance of dried specimens, or by supposed analogies with other vegetable or animal productions, rather than by patient dissection of the animal in its natural state. Cuvier states in his Regne Animal (t. iv. p. 87.), that the sponge is a fleshy substance, possessing no axis, either calcareous or horny; although we shall find, that, in one great tribe of these zoophytes, with spicula of complex forms, the axis is entirely calcareous and soluble, with effervescence, in acids; and it is well known, that the horny axis, of several sponges, have been constantly employed in the arts since the time of Alexander the Great, if not since the period of the Trojan war. Professor Schweigger of Konigsberg, who examined these animals alive, principally at Nice, believes that their axis consists of fibres which possess a small degree of irritability, by which they gradually contract the dimensions of the animal when it is irritated, and thus force out the water from its canals (*Beob. auf. N. R.* 1819, p. 33.); although, in his experiments, he could not excite them to the slightest perceptible motion; and in most of the known species these fibres are composed of minute siliceous tubes, which scratch glass and resist the action of the blowpipe. Lamarck, reasoning from mere analogy, maintains, that every species of sponge possesses distinct polypi, closely resembling those of alcyonia, projecting from its surface; and that these two genera of zoophytes differ only in the greater or less density of their gelatinous matter (*An. sans Vert.* t. ii. p. 348–9.); although his countryman Jussieu, nearly a century ago, by desire of the French Academy, examined with the microscope the *Spongia ramosa*, fresh from the rocks on the coast of France, and reported, that he could discover no kind of polypi in that animal (*Mem. de l'Ac.* 1742); and the accuracy of Jussieu's observations has been confirmed on a great variety of sponges, by every succeeding observer, as by Cavolini, Lamouroux, Schweigger, &c. It was scarcely consistent in Cavolini to consider the gelatinous matter as the muscular system of this animal (*Abhand. uber Pflanz-th. Sprengel's edit.* p. 124–6.), after he had repeatedly tried in vain to excite it to contract. One naturalist, well acquainted with the characters and habits of these animals, infers from analogy, that they possess nerves (*Phil. of Zool.* vol. i. p. 45.); while another, who has likewise studied them in the living and dried state, maintains, that they are animals which possess no organ whatever, either for growth or generation (*Lamouroux Hist. des Polyp.* p. 14). From observing the canals of the sponge constantly empty, or filled only with water, Lichtenstein was led to believe this substance to be merely a dead mass of the empty tubes of alcyonia, remaining after the decayed polypi had been washed out (*Skriv. af Nat. Sel. Kiob.* 1794). Blumenbach, and some other naturalists, apparently not aware of the close similarity of the fibrous axis of the sponge to that of some zoophytes, already known to possess polypi, and its dissimilarity to that of any known plant, and obviously not acquainted with the rapid currents and feculent discharges from its orifices, described by Ellis, Schweigger, Bell, &c. still regard the sponge as a plant, and consequently destitute of nerves, and muscular system, and polypi, and every kind of spontaneous motion, (*Blum. Nat. Hist.* 1825). This singular discordance of opinion among eminent naturalists of the present day, shows how little is yet known of the living organization and functions of this zoophyte, and the interesting field of discovery which lies open to those who love nature, and frequent the shores of the ocean.

In all the sponges I have met with alive, a distinct, soft, transparent matter, can be observed between the fibres; in some species, as the *S. panicea*, this matter is abundant and ropy; in others, as the *S. papillaris* and *coalita*, it is much thinner; and in others, as the *compressa* and *oculata*, it is found in smaller quantity. Probably no organized body can exist without similar soft parts. The fibrous part being always insoluble in water, can easily be procured separate from the soft matter, by immersing it repeatedly in hot water; it forms a net-work through every part of the body, and constitutes the *axis* or skeleton of this zoophyte, serving, as in other animals, to give form to the body, and support and protection to the softer organs. The axis is the part employed in the arts, or preserved in the cabinets of naturalists; it is the part of the animal which remains in a fossil state in the earth, as in the numerous fossil species found near Caen in France, (*Lamx. Exp. Meth.*); and it is that from which Aristotle and his successors have constantly taken the characters of the species. The structure of this part, or indeed of any other part of the sponge, cannot be observed without the assistance of the microscope; and it is well known that most zoophytes were regarded as plants, till the microscope reformed this part of science. But the minutest microscopical examination of the dried skeleton will not suffice alone to explain the living functions, or establish the nature of this animal. Lamarck, however, appears to have been misled by dried specimens or plates, or by preconceived hypothesis, in placing among the species of alcyonium the *Spongia cristata*, *S. tomentosa* or *urens*, *S. panicea*, and *S. palmata* of Ellis, which are common and well marked sponges, inhabiting our own coasts; and the *Spongia clavata* of Esper, which he has ranked as a variety of the *Alcyonium distortum*, has been lately shown by Schweigger to be a species of sponge resembling in texture the *S. oculata* (*Beob.* p. 29).

The axis differs so entirely in its nature in different sponges, that the living properties observed in one species, ought with very great caution to be extended to any other, and naturalists may probably take advantage of this difference, in classifying or subdividing this numerous and obscure tribe. In some species as the *S. communis*, *usitatissima*, *lacinulosa*, *fulva*, *fistulosa*, the axis consists only of cylindrical tubular *horny* fibres, which dissolve without effervescence in acids, leave no trace when rubbed on glass, and consume like hair when burnt, emitting the same horny odour. In others, as the *S. compressa*, *nivea*, (a small sessile species with triradiate, quadriradiate, and simple spicula, to be noticed hereafter, which I have so named from its beautiful white colour), *botryoides*, *coronata*, *pulverulenta*, the skeleton consists entirely of *calcareous* spicula, which disappear before the blowpipe, do not scratch glass, and dissolve with effervescence in nitric, sulphuric, and muriatic acids. And in others as the *S. cristata*, *papillaris*, *tomentosa*, *panicea*, *coalita*, *oculata*, *dichotoma*, *stuposa*, *alcicornis*, *compacta*, *fruticosa*, *parasitica*, *hirsuta*, *palmata*, *infundibuliformis*, *ventilabrum*, *hispida*, *suberica*, *nodosa*, we observe neither the horny tubular fibres of the first variety, nor the calcareous spicula of the second, but their whole axis is composed of minute *siliceous* tubular spicula, which, in dried specimens, appear drawn together into a longitudinal direction by the hardening of their connecting matter; these spicula scratch glass, do not dissolve in the above acids, nor consume by the blowpipe. The siliceous species abound on our shores, the calcareous are more rare, and I am not aware that any of the horny sponges has ever been observed so far north as the British shores.

Every one is familiar with the softness and remarkable elasticity of the common sponge, *S. communis*, which is the best example of the *horny* kind of axis. When a piece of it is brought near the flame of a candle, its fibres coil up, melt, and consume to a very small, light ash, with a horny smell, like hair; when a portion of it, well washed from sandy particles, is rubbed with a wooden instrument on glass, it leaves no perceptible streaks; when thrown into sulphuric or nitric acid, it diminishes in size, softens, and dissolves, without effervescence, into a brown pulpy matter, like other horny substances, and no spiculum is observed in the dissolved matter or precipitated to the bottom. Its fibres, and every thing of this nature, are best examined through the microscope, when they are suspended in water, and viewed by transmitted light. In this manner we observe them to be regularly cylindrical, translucent, of a brownish yellow colour, smooth on their external surface, all nearly of the same diameter, and distinctly tubular; they are tough, flexible, very elastic, generally quite straight, and they anastomose freely and completely with each other, through the whole body of the animal. Their diameter is about a third of that of a human hair, their length between their points of union varies from a tenth of a line to a line, and their internal tubular cavity occupies about half of their diameter, so that these horny fibres have a close resemblance to the spicula of many other sponges. From the clearness of the light transmitted through their central part; their internal cavity appears to be empty, which is not the case in the *S. fulva* and *fistulosa.* They unite at all angles, and they are a little dilated at their points of union; their internal cavities open freely into each other, and a small angular reservoir is formed at the place where they meet; they have no intervening connecting matter, no line of separation can be discovered at the angles where they pass into each other, and no opening is perceptible leading from their surface into their internal cavities; so that there is a continuous shut cavity in the interior of the fibres throughout the body of the largest common sponge, and these horny tubes winding round the pores and canals, cannot, therefore, be the cells of any kind of polypi, destined to create currents or other motions within the canals of this animal. The fibres unite so as to form polygons, whose sides lie almost always in different planes. The great elasticity of the axis shews that the orifices and canals, so obvious in this species, could not have been formed and left permanent, by any marine worms or insects merely traversing its texture; but must have formed a part of its original structure. The internal cavity of the strong horny fibres of the *S. fistulosa* and *S. fulva*, is completely filled with a dark granular opaque matter, which is continued from one fibre into another. This opaque matter renders the limits of the tubular cavity very distinct, and probably is the cause of these fibres being so remarkably hard and brittle, compared with the empty tubular fibers of the *S. communis.* The fibres of this last species, when highly magnified, resemble the empty stems of dead sertulariæ, from whose central axis the granular matter has been washed out, or consumed by animalcules, while the fibres of the two former species resemble the stems of living sertulariæ, whose central cavity is always filled with soft, moving, granular bodies.

ART. XX.—*A concise Statement of the Magnetical and other Philosophical Experiments and Observations made during the recent Northern Expedition under Captains Parry and Hopner* 1824-5. By a Correspondent.

IT was stated in our last Number, that the papers containing the detail of the above experiments were in the hands of the Admiralty. They have since been laid before the Royal Society, by whom they are expected to be published, forming an additional part, as was done last year in the case of Messrs Herschel and South's paper, containing their observations on the Double Stars. The expence of both being defrayed by the Board of Longitude.

The first and most extended paper is by Lieutenant Foster, containing a detail of his observations on the length of the seconds' pendulum, with the instrument which Captain Sabine employed in the numerous observations he made in various parts of the northern hemisphere. The nature of these observations is too well known to require any description of them in this place; but with respect to the observer, it may he proper to state, that he is the gentleman who accompanied and assisted Captain Basil Hall in his interesting voyage to the western coast of America, and who afterwards assisted Captain Clavering in his voyage to Spitzbergen and the eastern coast of Greenland, and whose accuracy as an observer, and indefatigable exertions, in every scientific pursuit, cannot fail henceforward to place his name amongst the most distinguished scientific navigators of England *.

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* We are glad to learn that Lieutenant Foster is at present engaged in preparing for another scientific voyage to the southward. He will accompany Captain King in the Endeavour.

The paper to which we now refer, contains the detail of four distinct series of pendulum observations; the first at the Royal Observatory, Greenwich, in May 1824, prior to the voyage; the second and third at Port Bowen in Prince Regent's Inlet where the Hecla and Fury wintered; and the fourth, which is indeed composed of two distinct sets, at the Royal Observatory on the return of the expedition.

The result of these experiments is in the highest degree satisfactory. The difference in the two Greenwich sets of observations, after an interval of eighteen months, under a different pressure, and after the pendulum had been exposed to a temperature of 47° below zero, is only that of two-tenths of a vibration in twenty-four hours; and the two series at Port Bowen give a still nearer approximation. In the former place, the observations were made in a room selected for the purpose by the astronomer royal; in the latter, in a snow-house, ingeniously constructed. By comparing the mean from the two series at each station, the author finds, for the ellipticity of the earth, $\frac{1}{309}$, which agrees well with a number of other independent observations, and is not very wide of the general deduction by Laplace, which is $\frac{1}{306}$, although it differs widely from the means deduced by Captain Sabine, which is $\frac{1}{289}$. It must, however, be carefully remembered, that this ellipticity of Captain Sabine is obtained by an accommodation of results, and is by no means directly deducible from his observations. It is merely that ellipticity which gives the least errors; and if we had any reason to believe that the earth was a spheroid of uniform ellipticity, the result obtained by Captain Sabine might be admissible; but is it not probable that different arcs have really different ellipticities? and if so, the mean obtained by encreasing the number of vibrations by *five* in one place, and decreasing them by the same number in another, in the space of twenty-four hours, must be considered unsatisfactory, if not erroneous. It is, in fact, assuming a uniformity of figure, which is at variance with all the best recorded experiments, amongst which those by Captain Sabine himself are justly included. We admire the accuracy of his experiments, but object altogether to his deductions; and we sincerely recommend to Lieutenant Foster, in all his future experiments, to observe the same rigid adherence to his experimental results as he has hitherto adopted, and not allow himself to speculate on accordances which may have no existence in nature.

Another extensive table of experiments, is a joint communication by Captain Parry and Lieutenant Foster, on the daily variation of the horizontal needle, in which they were assisted by most of the officers in the expedition. These experiments were commenced the 1st of January 1825, and continued, by hourly observations, to June; the results are highly curious and interesting. The daily variation of the needle in England ranges from about 6 or 7 minutes of a degree to 15 minutes; the former being the quantity due to the winter months, and the latter to the summer. It is, however, in either case but an inconsiderable quantity, and without great care, and very delicate suspensions, is not easily observed. Mr Barlow, some time back, proposed to increase this daily motion, by diminishing the directive powers of the needle by the application of other magnets, and having succeeded by this means in rendering it a very observable quantity, it became desirable to trace this motion in other and higher latitudes; and, accordingly, it furnished a very favourite pursuit to Captain Parry, Lieutenant Foster, and the other officers of the expedition. The apparatus was erected in December, and it was soon found, that instead of a variation of 6 or 7 minutes, as we have stated, to be the quantity at that season in England, they had a daily motion of nearly as many degrees, without using the means which were obliged to be had recourse to in England to increase the amount. In short, the needle was in a perpetual state of vibration, but still following a certain order in its motion, and which increased as the sun advanced to the summer solstice. The dip of the needle at Port Bowen was 88°, consequently the directive power of the needle was very small, and being in almost constant motion, it was impossible to ascertain the true mean magnetic meridian; but it was observed by Mr Hooper (who had made a graphical representation of the motion, according to a plan which Mr Christie had employed on a former occasion), that there was *only one* meridian, out of the many which the needle had traversed, which had been passed every day during the needle's motion; and for this reason the preference was given to this, and it was accordingly assumed as the true magnetic meridian. The mean time at which the needle traversed this meridian, going to the westward, was about five o'clock in the afternoon, and going eastward about six o'clock in the morning. The greatest easterly deviation happened at about ten o'clock in the morning, and the greatest westerly about the same time in the evening, observing that we here speak of the true easterly and westerly points, and not of the magnetic east and west; we ought, perhaps, rather to have said, that the greatest westerly magnetic bearing was at ten o'clock in the morning, and the greatest easterly at ten o'clock in the evening, for the mean variations being at Port Bowen about 124° westerly, the true and magnetic points were nearly reversed. The daily motion of the sun was obviously a primary cause of this daily variation, because it increased as the power of the sun increased; but it was very considerable even while the latter made its whole daily revolution below the horizon; and when it afterwards never sunk below the horizon, the character of the daily variations was preserved, the only change having been in the amount which was considerably greater in the latter case than in the former. It is the opinion, also, both of Captain Parry and of Lieutenant Foster, that some part of the observed changes was owing to the influence of the moon; the mean daily motion having been found uniformly greater at the time of conjunction than in quadrature or opposition.

In the experiments before alluded to by Mr Barlow, the needle was, by means of his neutralizing magnets, held at various points of the compass, in order, if possible, to trace out the direction of the force which produced the daily change in the direction of the needle; and he found a line about 16° to the west of the magnetic north, in which, when the needle was placed, there was no daily motion; or, at least, the motion was then at its minimum. Similar experiments were made at Port Bowen by Lieutenant Foster; and having carefully neutralized the needle, instead of a daily motion of 5° and 6°, he now obtained, in some positions, a variation of 50° and 60°, decreasing, as in Mr Barlow's, towards a minimum. In the present case, the line of no daily motion was about 84° from the meridian, and the order of the motion on each side of this line, as in those above referred to, was reversed, the needle on one side of this line passing to the right, and on the other side to the left, at the same hour of the day; and it is remarkable that the position of this line, as referred to the true meridian, has precisely the same bearing as in England; viz. that is about N. 40° west. Besides these daily changes in the direction of the horizontal needle, it was found that its intensity also experienced a very considerable change; and observations were accordingly instituted relative to that inquiry, and continued hourly for several months. These were performed by registering the time which the needle required to perform a certain number of vibrations; and which time varied from 17 to 18 minutes, increasing and decreasing regularly twice in the day with the variation. A similar change is known to obtain in Europe; but it is very inconsiderable. It appears, therefore, that both the daily variation in direction and in intensity, are dependent on the same cause; and that this cause, whatever it may be, operates much more powerfully in places where the dip is great, than in others where it is less considerable, as in England, France, &c.

We understand that Lieutenant Foster has still another communication to lay before the Royal Society, which is intended to point towards the cause of these various changes; and which is founded on a comparison of simultaneous observations on the intensity of the dipping and horizontal needle; but we are unacquainted with the results and deductions of this ingenious and accurate observer on this particular subject. The inquiry is one of great interest; and we are glad it has fallen into such able hands. If the cause in this case can be satisfactorily traced, we feel assured that terrestrial magnetism will soon be placed upon a level with most of the other physico-mathematical sciences. Should this be the case, although no other result had been obtained by the recent expedition, we should consider that a full reward had been secured for all the labours and expences attending this otherwise unfortunate voyage.

In concluding this brief notice, it is but justice to state, that the communications referred to above, although delivered only in the names of Captain Parry and Lieutenant Foster, may be almost considered as the joint labours of all the officers of the expedition. When we consider that the operations were carried on at a considerable distance from the ship, in a temperature frequently 40° and 47° below zero, with the sun for a considerable part of the time constantly below the horizon; and that notwithstanding these impediments, we have hourly observations, day and night, for nearly six months, it must be obvious, that the views of the two leading observers must have been cheerfully seconded by every officer; and we are pleased to observe, on this point, the most cordial and liberal acknowledgment on the part of the authors, of their obligation to Captain Hopner, to Mr Hooper, and to the officers in general, for their valuable assistance.

Besides the above communications to the Royal Society, several other experiments and observations were made; viz. on the application of Barlow's correcting plate; on the refraction of the atmosphere; on Daniel's hygrometers on the radiation of heat and the velocity of sound, which will be published in the appendix to the Account of the Voyage, at present in the press, by Captain Parry.

Art. XXI.—*Meteorological Observations made at Leith.* By Messrs Coldstream and Foggo.

The journal, from which the following monthly results are extracted, is kept about 20 feet above the level of the sea, and a few hundred yards distant from it. The Thermometer is registered at 9 A. M. and 9 P. M.; the Barometer at 9 A. M. Noon, 4 P. M. and 9 P. M.; the Rain-Gauge and Wind-Vane at Noon. The Hygrometrical observations are made by means of two Thermometers, one of which has its bulb covered with silk, and moistened with water; their indications are registered at noon.

DECEMBER 1825.

Results.

1. Temperature.	Fahr. Ther.
Mean of the month,	39°.295
Maximum by Register Thermometer,	51.500
Minimum by ditto,	26.000
Range,	25.500
Mean of the extremes,	38.750
2. Pressure.	Inches.
Mean of the month,	29.447
Maximum observed,	29.850
Minimum observed,	28.750
Range,	1.100

3. Humidity. Fahr. Ther.

Mean difference during the month between the two Thermometers,.. 1°.37

Maximum ditto,.. 2.30

Minimum ditto,.. 0.20

4. Rain,.. 2.34 inches in 15 days.

5. Winds, ... N. 4, NE. 1, E. 7, SE. 5, S. 1. SW. 5, W. 5, NW. 2, Var. 1,......days.

Remarks.

No phenomena of particular interest have occurred during December. The pressure has been upon the whole low; and the temperature, winds, and rain, have been moderate.

At 5 P. M. of the 14th, a thunder storm was experienced in many districts in Scotland, especially in Fifeshire; where the lightning killed several cattle, and set fire to some stacks of hay. In England, the same storm seems to have extended its ravages very widely: it was perhaps most severely felt about Northampton, Leicester, and Doncaster. Here, the pressure on that day was very low. At 9 A. M. the barometrical column stood at 29.05, whence it descended to 28.75 in the afternoon, and rose again a few tenths in the evening. The winds were variable, but chiefly E. and SW. very strong. Mean temperature of the day 41°,0; minimum 36°,5; maximum 45°,5.

The last ten days of the month were very pleasant; pressure moderate. Temperature about 33°,5. Winds N. and W. On the 27th, at noon, the force of solar radiation was 31°, the temperature of the air being 34°; that indicated by the black thermometer exposed to the sun's rays 65°. A little snow fell on the low grounds on the 29th and 30th; the neighbouring hills having been covered for some days previous.

ANNUAL RESULTS.

We have thrown the principal results afforded by our journal for 1826, into the annexed *Table*; to illustrate which, we think it may be proper to take a general survey of the meteorological history of the year; such a survey or running commentary (if we may be allowed the expression), being better calculated than mere numerical detail to interest practical men, and to induce them to pay that attention to meteorology, which its importance to the best interests of our race seems to claim for it, not as a matter of a few minutes notice only, day after day, but as a science, evidently capable of the greatest improvements, from the lights of modern philosophy. It is indeed gratifying to observe, in the pages of some contemporary Journals, strong evidence of a spirit for careful meteorological research diffusing itself over the country, and that those who have already imbibed this spirit, are the very men, who, of all others, are the best qualified, from the advantages of situation and occupation, to advance the science; and undoubtedly they will do so, if they pay that attention to it which it requires. We allude to the agriculturists of Scotland; and we hope that they will continue to improve their means of research, and not rest satisfied with trusting in the popular and erroneous opinions still abroad concerning the phenomena and laws of atmospheric variations, and which, except they who have by far the best opportunities for observation correct them, will never be investigated by philosophers.

During the last three months of the year 1824, the weather was particularly stormy; a very large quantity of rain fell, and the winds were unusually boisterous; but the commencement of 1825 ushered in a new state of things; the violence of the winds gradually abated; the pressure, which, during the preceding months, had been very low, increased rapidly, and rose unprecedentedly high; and the temperature was much elevated for the season: it rained during January on 11 days. February was a very pleasant month, mild and dry; pressure remarkably steady for the season, and gradual in its variations. No storms of wind occurred. Only 0.8 of an inch of rain fell; and the frosts, even in upland districts, were so slight, as scarcely to prevent the plough continuing its progress, except for a day or two. On the 26th, in the south of Scotland, there was a slight fall of snow, and another on the 28th. March was remarkable on account of the long period of dry weather which occurred. During the whole month, only 0.2 of an inch of rain fell: the pressure was very steady, and high. Temperature about the usual mean. The sun's rays were sometimes very powerful: their maximum effect observed was 58°,5, which is very high for the season. Mr Daniell, in the course of three years' observations, never saw the force of solar radiation exceed 49° in March. About the beginning of the month, there was a little snow, which lay for a few days on the hills, but quickly vanished from the low grounds.

In April, there were only 6 wet days, and only 0.2 of an inch of rain, so that the ground got quite dry, the effect of the excessive rains in 1824 being completely annihilated. West winds prevailed during the first 20 days, and east during the remainder of the month. "Owing to this very favourable weather, there was more than the usual proportion of spring wheat sown. All the grain crops were in the ground before May, and they never got a drier bed. A more favourable lambing season could not have been wished for."

In the beginning of May, vegetation was far advanced: in many parts of Scotland it was said to be 15 or 20 days earlier than usual. The distinguishing character of the month was the prevalence of easterly winds, these having blown rather strongly for 22 days. A little rain fell during the first week, but none again till the 25th. On the 28th, all the neighbouring hills were covered with snow: about 0.40 of an inch of rain had fallen the day before on the low lands.

The weather during June was variable: the sky was frequently obscured by dense clouds. Temperature and pressure moderate; winds variable. The seasonable intervals of bright sunshine, and the genial moisture, raised a most luxuriant growth of every kind of farm crop, and gave to the horticulturist the brightest prospects of a well stocked orchard.

July was particularly characterised by the prevalence of unusually high temperatures, and a long continuance of dry weather. On the 1st, 10th and 15th, heavy rain fell, but none during the rest of the month. The winds were variable, both in direction and strength. It was after the 15th that the temperature began to be oppressive. Here, the thermometer was not observed above 81° in the shade; but in many inland situations it was seen above 85°. It is certain, at least, that, throughout the whole of Scotland, the mean temperature of the atmosphere was for several days above 70°, a degree of heat rarely experienced in this country. The force of solar radiation during this period was also very great. We observed it several times to exceed 65°; and on the 27th it was 75°, the covered thermometer having risen in the sun-beams to 150°. The consequence of this excessive heat was, that the country was "burnt up;" and in many districts the crops were brought to a premature harvest. "Up till the middle of June, the season was the finest ever recollected; at that period, if there ever was as great, there certainly never was a greater promise of crop in the country; but the want, not only of rain, but also of dew, since that time, has greatly curtailed our prospects." The following relates to Perthshire: "At the end of April, the soil was, for the most part, tolerably well saturated with moisture. A regular and moderate supply of rain in May, afforded sufficient moisture to the growing crops; but about the 8th of June, the heat began to be oppressive, and the rains less frequent. July passed with scarce any rain, while the temperature was unusually high. On the 27th, the thermometer stood at 87° in the shade, an elevation which it has not reached in Perthshire for twelve years before; nor during the same period have the rains been so limited. In the northern parts of the county, indeed, thunder showers were frequent, and the soil was liberally supplied with moisture; but in all the southern districts, the drought was most severe. On light gravelly soils, the crop will be very short, and the extreme heat, with clear sunshine, is bringing on a premature ripeness. In the early districts, the pastures are completely burnt up."

The crops derived the greatest advantage from heavy rains which fell during the first two weeks of August, while the remainder of the month was as favourable to the operations of harvest as could be wished: the weather was steady, no rain fell; and the radiation from the sun was direct and powerful. The mean temperature of the month was 58°,2, and more than 2 inches of rain were measured. The autumnal diseases prevailed towards the latter end of the month, to a very great extent in many districts; and on the whole, the season may be said to have been a sickly one.

September was a pleasant month, and was favourable for the most part to field operations. The pressure was rather low, and the humidity considerable, although less rain fell than during the preceding month.

In October, there was a great prevalence of strong westerly gales, accompanied during the first two weeks by heavy rains, and towards the end of the month by frosts. The temperature was above the mean; the pressure moderate. Rain fell on 20 days to the depth of 2.6 inches. The harvest was completed beautifully, and most orchard fruits were abundant.

November.—The temperature about the mean; pressure low; west winds prevalent. A considerable number of auroræ were seen during this month. The minimum temperature was 25°. The year closed with moderately pleasant weather. The winds during December were variable, but not particularly strong. The humidity was not great; 2.3 inches of rain fell.

The whole year may be characterized as having been warm and dry. The annual mean temperature is not, indeed, much above the average; but the quantity of rain is particularly small, being only 17.8 inches.

February 1826.

ANNUAL RESULTS of the Meteorological Journal kept at Leith by Messrs Coldstream and Foggo.

1825.	TEMPERATURE.					PRESSURE.					HUMIDITY.				RAIN.			Maxima of the Force of Solar Radiation.	WINDS.								
	Monthly Means.	Maxima.	Minima.	Monthly Ranges.	Greatest daily Ranges.	Mont'ly Means.	Maxima.	Minima.	Monthly Ranges.	Greatest daily Ranges.	Monthly Means.	Maxima.	Minima.	Dew-point. Monthly Means.	Monthly Quantities.	Number of Fair days.	Number of Wet days.		N.	NE.	E.	SE.	S.	SW.	W.	NW.	Variable.
	°	°	°	°	°						°	°	°	°	Inches.			°									...
January,...	40.975	60.5	25.0	35.5	19.0	29.995	30.90	28.80	2.10	0.55	3.84	6.50	0.25	...	1.40	20	11	...	...	2	...	...	...	25	2	2	...
February,	39.625	57.0	21.5	35.5	16.5	29.956	30.41	29.14	1.27	0.61	2.99	5.75	0.75	...	0.86	17	11	27.0	...	1	...	6	...	13	5	3	...
March, ...	39.790	61.0	29.0	32.0	27.0	30.048	30.62	29.13	1.49	0.41	2.61	3.90	0.50	...	0.20	26	5	58.5	...	3	3	8	4	7	4	2	...
April, ...	45.891	64.0	31.5	32.5	25.5	29.978	30.50	29.30	1.20	0.34	5.25	9.20	0.50	...	1.23	24	6	56.0	...	...	8	2	...	6	10	4	...
May, ...	48.000	70.0	36.0	34.0	23.0	29.801	30.30	29.50	0.80	0.20	5.72	11.70	0.60	...	2.01	19	12	...	2	...	22	...	...	...	4	3	...
June, ...	56.525	76.0	40.5	35.5	24.0	29.826	30.35	29.15	1.20	0.22	5.94	11.30	2.50	...	1.24	13	17	62.0	3	2	3	1	3	5	6	3	4
July, ...	60.960	81.0	45.0	36.0	27.0	30.081	30.30	29.72	0.58	0.24	6.97	14.70	0.00	52.3	0.07	28	3	82.0	1	1	11	2	4	1	4	1	6
August, ...	58.209	77.0	44.0	33.5	21.0	29.806	30.25	29.03	1.22	0.21	6.53	11.00	2.90	...	2.23	17	14	...	1	5	5	...	...	2	14	2	2
September,	57.260	69.0	40.0	29.0	22.0	29.742	30.30	29.30	1.00	0.47	4.70	13.00	0.00	46.5	1.32	16	14	67.0	...	3	4	1	...	4	8	1	9
October,...	51.241	68.0	33.5	34.5	19.0	29.738	30.25	29.00	1.25	0.51	3.50	8.50	0.00	43.0	2.60	11	20	45.5	2	1	...	...	4	7	11	6	...
November,	39.850	56.5	25.0	31.5	22.5	29.482	30.12	28.67	1.45	0.62	2.70	4.50	0.00	37.0	1.97	13	17	25.0	2	...	2	...	...	4	14	5	3
December,	39.295	51.5	26.0	25.5	15.5	29.447	29.85	28.75	1.10	0.50	1.37	2.30	0.20	34.2	2.34	16	15	22.5	4	1	7	5	1	5	5	2	1
ANNUAL Means & Sums, ...	48.135	66.04	33.08	32.87	21.83	29.860	30.90	28.67	1.221	0.62	4.55	14.7	0.00	...	17.47	220	145	82.0	15	19	65	25	16	79	87	34	25
		81.00	21.50	59.50	27.00																						

ART. XXII.—*Celestial Phenomena from April 1. to July 1. 1826, calculated for the Meridian of Edinburgh, Mean Time.* By Mr GEORGE INNES, Aberdeen.

The times are inserted according to the Civil reckoning, the day beginning at midnight.—The Conjunctions of the Moon with the Stars are given in *Right Ascension*.

APRIL.

D.	H.	′	″	
4.				☿ greatest elong.
7.	1	8	56	Em. II. sat. ♃
7.	1	58	3	Em. I. sat. ♃
7.	9	15	46	● New Moon.
8.	0	57	40	☌ ☽ ♀
8.	20	26	38	Em. I. sat. ♃
9.	1	8	11	☌ ☽ ☿
10.	0	41	49	☌ ☽ δ ♈
11.	2	11	0	☌ ☽ A ♉
11.	11	57	47	☌ ☽ 2 κ ♉
12.	5	36	10	☌ ☽ ι ♉
12.	13	29	51	☌ ☽ ♄
12.	21	37	9	☌ ☽ ζ ♉
13.	21	4	40	☌ ☽ ν ♊
15.	12	45	18	☽ First Quarter.
15.	19	53	46	Im. IV. sat. ♃
15.	22	10	44	Em. III. sat. ♃
15.	22	21	1	Em. I. sat. ♃
15.	23	55	46	Em. IV. sat. ♃
16.	17	10	23	☌ ☽ 1 α ♋
16.	18	20	48	☌ ☽ 2 α ♋
17.	3	50	0	☌ ☿ ♀
18.	11	25	25	☌ ☽ ♃
20.	15	29	34	☉ enters ♉
21.	16	39	48	☌ ☽ i ♍
22.	7	12	11	○ Full Moon.
22.	22	53	0	Im. III. sat. ♃
23.	0	15	28	Em. I. sat. ♃
23.	2	8	48	Em. III. sat. ♃
23.	9	9	40	☌ ☽ ♂
23.	10	57	10	☌ ☽ κ ♎
24.	1	23	20	☌ ☽ λ ♎
24.	5	45	40	☌ ☽ 1 β ♏
24.	5	47	0	☌ ☽ 2 β ♏
24.	14	0	0	Inf. ☌ ☉ ☿
25.	10	15	42	☌ ☽ ρ Oph.
26.	6	46	34	♂ ☽ 1 μ ♐
26.	7	21	49	☌ ☽ 2 μ ♐
26.	11	35	22	☌ ☽ ♅
27.	8	54	40	☌ ☽ d ♐
28.	12	58	21	☌ ☽ β ♑
29.	6	47	53	☾ Last Quarter.

MAY.

D.	H.	′	″	
1.	20	38	37	Em. I. sat. ♃
1.	22	18	41	Em. II. sat. ♃
4.	18	43	16	☍ ☉ ♂
5.	1	59	25	☌ ☽ ☿
7.	2	3	19	● New Moon.
7.	23	22	15	☌ ☽ ♀
8.	8	7	6	☌ ☽ A ♉
8.	10	33	12	Em. I. sat. ♃
8.	17	52	44	☌ ☽ 2 κ ♉
9.	0	56	1	Em. II. sat. ♃
9.	11	29	0	☌ ☽ ι ♉
9.	12	5	6	☌ ☽ ♄
10.	3	29	16	☌ ☽ ζ ♉
11.	3	11	47	☌ ☽ ν ♊
14.	0	9	26	☌ ☽ 1 α ♋
14.	1	21	37	☌ ☽ 2 α ♋
14.	23	55	29	☽ First Quarter.
15.	7	42	40	☌ ☽ ♃
16.	0	27	48	Em. I. sat. ♃
19.	3	14	2	☌ ☽ i ♍
19.	17	47	43	☌ ☽ ♂
21.	7	54	50	☌ ☽ κ ♎
21.	12	9	26	☌ ☽ λ ♎
21.	14	59	55	○ Full Moon.
21.	15	51	2	☉ enters ♊
21.	16	38	29	☌ ☽ 1 β ♏
21.	16	40	47	☌ ☽ 2 β ♏
22.				☿ greatest elong.
22.	20	43	31	☌ ☽ ρ Oph.
23.	16	45	4	☌ ☽ 1 μ ♐
23.	17	19	20	☌ ☽ 2 μ ♐
24.	18	8	6	☌ ☽ d ♐
24.	19	42	0	☌ ☽ ♅
24.	20	51	7	Em. I. sat. ♃
24.	21	21	38	☌ ♀ ♄
25.	21	10	47	☌ ☽ β ♑
28.	13	30	23	☾ Last Quarter,
28.	22	2	8	Em. III. sat. ♃
31.	22	45	47	Em. I. sat. ♃

JUNE.

D.	H.	′	″	
3.	9	45	0	☌ ☽ ♀
3.	12	56	40	☌ ☽ δ ♈
4.	14	17	27	☌ ☽ A ♉
5.	0	0	47	☌ ☽ 2 κ ♉
5.	17	39	38	● New Moon.
6.	9	24	40	☌ ☽ ζ ♉
6.	12	50	57	☌ ☽ ♄
7.	8	57	50	☌ ☽ ν ♊
7.	18	24	50	☌ ☽ ♀
10.	5	44	40	☌ ☽ 1 α ♋
10.	6	57	16	☌ ☽ 2 α ♋
12.	5	47	0	☌ ☽ ♃
13.	7	41	16	☽ First Quarter.
15.	11	45	10	☌ ☽ ι ♍
16.	8	54	48	☌ ☽ ♂
17.	2	49	0	☌ ☉ ♄
17.	17	57	6	☌ ☽ κ ♎
17.	22	16	20	☌ ☽ λ ♎
18.	2	50	42	☌ ☽ 1 β ♏
18.	2	52	0	☌ ☽ 2 β ♏
19.	7	13	48	☌ ☽ ρ Oph.
19.	22	41	50	○ Full Moon.
20.	3	15	0	☌ ☽ 1 μ ♐
20.	3	49	5	☌ ☽ 2 μ ♐
21.	4	23	0	☌ ☿ ♄
21.	16	0	0	☌ ☽ d ♐
21.	16	15	8	☌ ☽ ♅
22.	0	32	16	☉ enters ♋
22.	6	55	16	☌ ☽ β ♑
24.	16	30	0	Sup. ☌ ☉ ☿
27.	4	45	51	☾ Last Quarter.
30.	19	49	14	☌ ☽ δ ♈

Times of the Planets passing the Meridian.

APRIL.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	13 10	12 26	2 43	21 54	16 25	7 7
5	13 11	12 29	2 27	21 37	16 11	6 49
10	13 3	12 32	2 4	21 16	15 53	6 30
15	12 44	12 36	1 41	20 55	15 34	6 11
20	12 41	12 40	1 17	20 34	15 18	5 52
25	11 53	12 45	0 52	20 14	15 1	5 33

MAY.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	11 16	12 50	0 19	19 51	14 40	5 10
5	10 57	12 55	23 54	19 36	14 27	4 55
10	10 39	13 1	23 26	19 16	14 8	4 34
15	10 27	13 7	22 59	18 56	13 51	4 14
20	10 21	13 13	23 23	18 39	13 34	3 54
25	10 20	13 21	22 11	18 20	13 18	3 33

JUNE.

	Mercury.	Venus.	Mars.	Jupiter.	Saturn.	Georgian.
D.	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
1	10 27	13 30	21 34	17 53	12 54	3 6
5	10 39	13 36	21 16	17 39	12 40	2 50
10	10 54	13 41	20 55	17 22	12 23	2 29
15	11 13	13 49	20 35	17 5	12 6	2 9
20	11 38	13 56	20 16	16 47	11 50	1 49
25	12 6	14 2	19 59	16 30	11 34	1 27

ART. XXII.—*List of Rare Plants which have Flowered in the Royal Botanic Garden, Edinburgh, during the last three months.* Communicated by Prof. GRAHAM.—*Mar.* 9. 1826.

Amaryllis psittacina.
Antennaria triplinervis.
Astrapæa Wallichii.
Atragene capensis.
Epidendrum elongatum.
Euonymus japonicus.
Goodia pubescens.
Jasminum hirsutum.
Limonia trifoliata.
Orontium aquaticum.
Pothos coriacea.

Roots strong, fleshy, round, and branched. Without *stem*. *Leaves* petioled, lanceolate, undulate, coriaceous, dull green, about 2½ feet long, suberect, set obliquely on the petiole, veined, having an obscure lateral rib near the edge of the leaf; middle rib very strong, prominent, and round both behind and before. *Petioles* rising from the centre of the crown of the root, where very turgid, 6–8 inches long, semicylindrical, about as thick as the fore-finger, with a thickened joint at the base of the leaf, and here the cuticle generally becomes wrinkled transversely, cracked, and brown. *Stipules* broad at the base, clasping the bases of several petioles, pointed, green, persistent, and becoming torn, withered, white. *Peduncles* erect. *Spatha* suberect, ovato-lanceolate, acuminate, pale green, rather shorter than the spadix. *Spadix* round, tapering, about 5 inches long, with the peduncle about half the length of the leaves and petiole, greenish-white, shortly after its evolution covered with globules of a transparent, colourless fluid, giving it in most lights a very beautiful silvery appearance. *Anthers* yellow; *filaments* white. *Pistil* white, spotted with rose-colour.

This species I have seen at Kew; but I am not aware that it has any where been described. The specific name here given refers to the firm, dry, thick foliage.

Pothos Harrisii.

Caulescent. *Roots* creeping, and, as they descend perpendicularly from many parts of the stem, cylindrical, fleshy, red, slightly scarred. *Stems* flexuose, jointed, green.

Leaves petioled, scattered, about 18 inches long, cordato-lanceolate, acute, bright green, shining, veined, somewhat folded in the middle, flat when beginning to decay; middle rib very strong, projecting both behind and before, in its upper half sharp before, round in its whole length behind; veins united at their extremities towards each edge of the leaf by a waved nerve, scarcely stronger than the veins. *Petiole* about 3 inches long, sometimes much longer, swollen at its insertion into the stem, and jointed close to the leaf, green, furrowed above, slightly winged, wing waved; *stipules* long, pointed, reddish-yellow, persisting, and with their remains forming a brown ragged sheath to the upper part of the stem.

Peduncle axillary, equal in length to the leaf and petiol, slender, erect. *Spadix* slightly tapered, about 5 inches long, greenish-brown. *Spatha* nearly as long as the spadix, narrow, pointed, reflected, pale green, reddish at the tip; *anthers* yellow; *filament* white; *pistil* pale green, spotted with red.

Brought with the *P. coriacea* by Captain Graham of H. M. Packet Service from Rio Janeiro, along with several other new and rare plants, in 1824. They were given to him by M. Joaquim Harris of Rio, in testimony of whose exertions in behalf of practical botany I have named the present species. Both are kept in the stove, and grow freely. Excellent figures by Dr Greville will soon be given in Hooker's Exotic Flora.

Xylopia muricata.

ART. XXIV.—*Proceedings of the Wernerian Natural History Society.* Continued from p. 165.

17th Dec. 1825.—THE Secretary read, 1. A paper by Mr John Murray, Lecturer on Chemistry, detailing some curious experiments and observations made by him on the varying temperature of the Chameleon, as connected with the changes of colour exhibited by the animal;—2. A notice by P. J. Selby, Esq. of Twizell House, regarding a specimen of the rare *Larus minutus* shot in Galloway; and, 3. A communication from Dr T. S. Traill of Liverpool, regarding the use of oil of turpentine for preserving zoological specimens in cabinets. (See p. 135. of this volume.)

At the same meeting, Professor Jameson read, 1. A notice of Zircon having been in primitive rocks in the island of Scalpay, by Mr William Nicol, Lecturer on Natural Philosophy; (printed in this volume, p. 138. *et seq.*); 2. Mr William Macgillivray's account of the animals of the classes *Cirripeda*, *Conchifera*, and *Mollusca*, observed in the Island of Harris; and, 3. A letter from Mr Meynell of Yarm, Yorkshire, on changing the habits of Fishes, and mentioning that he had, for four years past, kept the smelt or spirling (*Salmo Eperlanus*, Lin.) in a fresh-water pond, having no communication with the sea, by means of the Tees or otherwise, and that the smelts had continued to thrive and breed as freely as when they enjoy intercourse with the sea.

14th Jan. 1826.—Professor Jameson read Mr Cormack's History of the Geographical Distribution and economical uses of some Fishes on the Banks of Newfoundland, with an account of the Great Seal Fishery of that station. Part of Mr Thomas Buchanan's essay on the Comparative Anatomy of the Organ of Hearing, was then read. (See p. 71. of this volume.)

Dr Knox, Lecturer on Anatomy, then read his account of the anatomy of the Wombat of Flinders.

At the same meeting, specimens of the Japan Peacock and Peahen, and of the galeated and undulated Hornbills, were exhibited and described by Professor Jameson; and Dr Fleming of Flisk, exhibited a specimen of the Migratory Pigeon of North America, shot in Fife on 31st December last; and shewed, from the perfect state of the plumage, that the animal had not been in a state of confinement, but had probably been wafted across the Atlantic by strong and continued westerly gales.

28th Jan.—At this meeting, there was read an account of Highland Alluvium, being the concluding part of an essay on Sandfields, in which the author extended his observations to the summits of primitive mountains.

Professor Jameson then read a communication, received from a foreign correspondent, on the probability that meteoric stones are formed in the atmosphere, and not derived from the moon, or any other extra-mundane source.—The Professor also shewed to the meeting several large specimens of Beryl from the Mountains of Morne in Iceland, and mentioned that they occur along with rock-crystal, in drusy cavities, in the granite composing these mountains.

ART. XXV.—SCIENTIFIC INTELLIGENCE.

ASTRONOMY.

1. *The Double Star*, 61 *Cygni.*—It appears by the proceedings of the Royal Institute of France, that M. Arago lately made a report of his observations for investigating whether this remarkable double star had a visible parallax. He failed in discovering a sensible parallax. Dr Brinkley long observed this star for the same purpose, and found no parallax in declination; and Mr Bessell also compared it with the neighbouring stars in right ascension, and his result was, that its parallax appeared even negative, seeming to shew that it was more distant than those stars. Dr Brinkley states, in his Elementary Treatise of Astronomy, "I have made observations of the zenith distances, at the opposite seasons, to endeavour to discover any sensible parallax in these stars, but there appears to be no sensible parallax." The rapid motion of this pair of stars certainly would induce us to believe them nearer than other stars, but this

notion, when examined, appears to be no better supported than the commonly received one, that the brightest stars are nearest to us.—*Dublin Philosophical Journal.*

2. *Opposite Effects of a Change of Density of the Air, as affecting the going of a Clock.*—Davies Gilbert, Esq. M. P. a short time ago published some ingenious investigations on the vibration of pendulums, and shewed, that on a change of an inch in the height of the barometer, an astronomical clock ought to change its rate, in consequence of the alteration in the buoyancy of the air, by two-tenths of a second a-day. Having applied to Mr Pond and Dr Brinkley to examine this point, he was surprised to find that they had discovered no such change. On reconsidering the subject, he finds a cause which before he had supposed too small to have any effect, almost exactly counteracting the effect of the change of buoyancy. This cause is the alteration of the arc by the altered resistance of the air. He remarks: "It is an extremely curious circumstance, that, without any reference to the attainment of this balance between opposite disturbing causes, our best clocks should have been fortuitously made to vibrate very nearly in the arc which reduces them to equality." For the mathematical investigations and tables illustrative of this singular coincidence, we must refer to the Quarterly Journal of Science for October.—*Dublin Philosophical Journal.*

3. *Local Attractions.*—The *Connaissance des Temps* 1827, contains an account of geodetical operations in Italy by the French geographical engineers, remarkable for the discordance it exhibits between results deduced from these operations, and from astronomical observations. Of the exactness of the survey no doubt can be entertained from the recital given, and the astronomical results are founded on the observations of several most able astronomers. The discordances, which in one case amount to nearly 27″, and in another to 17″, are attributed to local deviations of the plumb-line, caused by irregular attraction. The matter near the surface at Milan appears to attract the plumb-line considerably to the north of the vertical, and that near Remini considerably to the south.—*Dublin Philosophical Journal.*

NATURAL PHILOSOPHY.

4. *Experiments on the Compression of Air and of Gases.*—These experiments were made by M. Oersted, with the assistance of M. Suenson. The most powerful compressions were made in the breech of an air-gun, in which they succeeded in compressing air to the 110th part of its original volume. It was found that Mariotte's law was preserved in these high pressures. In their next experiments, which were made on gases, they succeeded in establishing the existence of the same law, even when these gases were about to be converted into liquids. M. Oersted remarks, that, in liquids, the compressions equally follow the proportion of the compressing force, and that it is extremely probable that solids are subject to the same law. He therefore concludes, that this simple law, That the diminution of volume is proportional to the compressing force, holds in each of the three classes of bodies. He adds, that this law can only be admitted on the supposition that the caloric developed by compression has been permitted to escape before the measurement is made.—*Dublin Philosophical Journal.*

METEOROLOGY.

5. *Magnetizing Power of Light.*—Mrs Mary Somerville, one of the most highly gifted and accomplished females of our time, has lately communicated to the Royal Society of London a memoir on the magnetizing power of the more refrangible rays of light. From the beautiful experiments detailed in the communication, Mrs Somerville infers, *that the more refrangible rays of light have the property of imparting magnetism.*

6. *Daniel on the Barometer.*—From a memoir of this distinguished observer, lately read before the Royal Society of London, it appears that he has established the following facts: 1. That air gradually insinuates itself into the best made barometers of the common construction. 2. That this does not take place from any solution of the air by mercury. 3. That the passage of the air is between the mercury and the glass. 4. That the gradual deterioration of barometers may be prevented by a ring of platinum cemented to the open end of the tube.

7. Meteorological Table, extracted from the Register kept at Kinfauns Castle, North Britain. Lat. 56° 23′ 30″. *Above the Level of the Sea* 140 *Feet.*

1824.	Morn. 10 o'clock. *Mean Height of* Barom.	Morn. 10 o'clock. *Mean Height of* Therm.	Even. 10 o'clock. *Mean Height of* Barom.	Even. 10 o'clock. *Mean Height of* Therm.	Mean Temp. by Six's Therm.	Depth of Rain in Inches.	No. of Days. Rain or Snow.	No. of Days. Fair.
January,	29.961	39.387	29.936	39.935	40.355	1.45	9	22
February,	29.912	39.928	29.893	39.250	40.071	0.95	9	19
March,	29.992	41.742	29.978	40.161	41.709	1.20	10	21
April,	29.854	47.300	29.835	43.600	46.700	2.40	9	21
May,	29.873	51.322	29.897	47.097	50.096	2.60	13	18
June,	29.785	57.566	29.764	53.000	56.500	2.50	9	21
July,	30.010	63.097	30.020	58.129	62.032	0.30	5	26
August,	29.733	61.322	29.725	57.485	60.838	2.00	9	22
September,	29.715	58.600	29.701	54.866	57.600	2.35	16	14
October,	29.678	51.322	29.671	48.903	55.161	2.15	14	17
November,	29.451	41.400	29.417	39.833	41.066	2.80	9	21
December,	29.412	40.677	29.437	40.484	40.451	3.20	17	14
Average,	29.781	49.742	29.773	46.895	49.048	23.90	129	236

ANNUAL RESULTS.

MORNING.

Barometer.				*Thermometer.*			
Highest,	9th Jan.	30.80	Wind SW.	Highest,	16th June,	71°	Wind SW.
Lowest,	18th Jan.	28.66	E.	Lowest.	31st Dec.	25	W.

EVENING.

Highest,	9th Jan.	30.75	Wind SW.	Highest,	30th July,	66°	Wind SE.
Lowest,	5th Nov.	28.64	SE.	Lowest,	31st Dec.	26	W.

Weather.	*Days.*	*Wind.*	*Time.*
Fair, - - -	236	N. & NE. - -	9
Rain or Snow, - -	129	E. & SE. - -	119
		S. & SW. - -	95
	365	W. & NW. - -	142
			365

Extreme Cold and Heat by Six's Thermometer.

Coldest, 31st December,	Wind W.	21°
Hottest, 18th July,	—— W.	80°
Mean Temperature for 1825,	-	49° 048′

Result of Two Rain Gauges.

	Inches.
1. Centre of Kinfauns Garden, about 20 feet above the level of the Sea, - - - - - -	23.90
2. Square Tower, Kinfauns Castle, about 140 feet, - -	23.45

8. *Luminous Meteor.*—On the 2d of January 1825, about 5 A. M., M. Antonio Brucalassi, on his return to Arezzo, observed, between S. Giovanni and Montevarchi, a singular electric phenomenon. About a hundred paces off, and at the height of ten fathoms, or less, from the ground, appeared, on a sudden, a luminous meteor, of the form of a truncated cone. This meteor appeared to be formed by a globe of fire situated in its fore part, which was the narrower, and which, by its rapid motion, left behind a track of light, which gave it the appearance of a cone. This light became gradually less intense towards the base, and seemed to be split into rays issuing from the opposite extremity. The whole surface of the cone was illuminated, and cast out sparks of the greatest brilliancy, in brightness like the electric sparks, but in the effect resembling those exhibited by filings of iron, when thrown upon the flame of a candle. The whole length of the meteor appeared to be about two fathoms, and the diameter of the base half a fathom. At the centre of this base, there was a total absence of light, which formed in that part a dark spot. The direction of its motion was from west to east, and nearly horizontal, inclining, however, a little towards the earth. Its motion was very rapid; for in less than five seconds it traversed a space of about 350 paces. During this passage it shed a most brilliant light, so that a certain extent of land was illuminated, as in full day-light. The emanations of this luminous body were lost in the air, instead of being extinguished in the ground; it left behind no smell; produced no explosion or noise of any kind, not even that hissing made by artificial fire-works. The night in which this phenomenon occurred was calm, but very cold, and the sky clear. A great number of shooting stars were seen before and after the appearance of the meteor.—*Antologia, Feb.* 1825.

GEOGRAPHY.

9. *Edinburgh Geographical and Historical Atlas.*—In our notice of this work in a former Number, we stated, incorrectly, that the letter-press was in octavo, whereas it is in folio. Two numbers have appeared, in which the learning and good sense displayed by the author in his history of Geography, augur well for the success of the work. We hope the author will,

in future maps, give more detailed representations of the discoveries of Franklin and Richardson, Parry and Scoresby, than those in the map in the second number. The important additions made to our knowledge of the antarctic lands, by a very intelligent and meritorious officer, Captain Weddel, ought also to be fully and carefully recorded.

10. *Distribution of Land and Water.*—From the unequal distribution of the continents and seas, the southern hemisphere has long been represented as eminently aquatic; but the same inequality makes its appearance, when we consider the globe divided, not in the direction of the Equator, but in that of the Meridians. The great masses of land are collected between the meridians of 10° to the west, and 150° to the east of Paris; while the peculiarly aquatic hemisphere commences to the westward, with the meridian of the coasts of Greenland, and terminates to the east with the meridian of the eastern shores of New Holland, and the Kurile Isles. This unequal distribution of the land and water, exerçises the greatest influence upon the distribution of heat at the surface of the globe, upon the inflexions of the isothermal lines, and upon the phenomena of climate in general. With reference to the inhabitants of the centre of Europe, the aquatic hemisphere may be called western, and the terrestrial hemisphere eastern, because in proceeding westward, we come sooner to the former than to the latter. Until the end of the 15th century, the western hemisphere was as little known to the inhabitants of the eastern hemisphere, as a half of the lunar globe is at present, and probably will always remain to us.—*Humboldt.*

11. *Iceland.*—According to the map in Gieman's description of Iceland, this island lies between 63° 23′, and 66° 33′ N. Lat. The surface of the country occupies 1.800 square miles. In 1824, the population was 50,092 souls. The whole of this population, extended over a considerable space, has but one physician and four surgeons; but 154 christian pastors.

MINERALOGY.

12. *Vesuvian (Idocrase) of Egg near Christiansand.*—The crystals of Vesuvian which are found at Egg, near Christiansand

in Norway, are distinguished from the crystals of the same species hitherto known, by their great size, being several inches in thickness, and half a foot, or perhaps more, in length. The terminal faces of moderate extent are so perfect that they leave nothing to be desired in this respect. But the most remarkable circumstance relating to these crystals, is their having a very distinct appearance of growth in their structure, the whole mass being divided into a succession of scales or envelopes covering one another. M. Weiss gives some illustrations regarding this structure, and then passes to the description of the new form which he has observed; it is derived from the fundamental prism by modifications on the longitudinal edges, on the edges and angles of the base, and appears to approach closely to that which Haüy has represented, by fig. 71. of his Treatise.

13. *New Analysis of the Steinheilite or Dichroite of Orijarvi, by P. A. Bonsdorff.*—The analysis of this substance has already been made by Professor Gadolin, whose investigation of it appeared in the Memoirs of the Imperial Academy of Sciences of Petersburg, accompanied by a very accurate description of the mineral by Count Steinheil. At the request of the same chemist, M. Bonsdorff has repeated the analysis, and has obtained the following result:

Silica,	49,95	containing	25.11 of oxygen.
Alumina,	32,88		15.35
Magnesia,	10,45		4.04
Oxide of Iron,	5.00		1.53
Oxide of Manganese,	0.03		
Volatile parts,	1.65		
	99.96		

This composition is represented by the formula $M\,\underline{S}^2+4\left\{\begin{matrix}A\\F\end{matrix}\right\}S$, according to which the following proportions have been calculated; silica, 49.93; alumina, 32.60; magnesia, 10.32; oxide of iron, 5.00.

14. *Phillipsite.*—It appears, from a late analysis of Gmelin, that the Harmotome of Marbourg contains potash in place of barytes, and therefore belongs to the species Phillipsite, described by Mr Levy. It is named by some German mineralogists Kali-harmotome.—*Bucklandite.* This mineral, so nearly allied to pistacite, has been met with in the rocks of the Lake of Laach.

15. *Tabular Spar of Pargas.*—Among the numerous and

remarkable minerals which are found in the limestone mountains of the parish of Pargas, in the neighbourhood of Abo, there is one of a radiated structure and white colour, which has been taken for a tremolite, but which should be referred to the Tafelspath of the Germans. According to the examination which M. Bonsdorff has made of it, in 100 parts it contains,

Silica,	52.58
Lime,	44.45
Magnesia,	0.68
Oxide of Iron,	1.13
Volatile parts,	0.99
	99.93

This mineral is therefore a bisilicate of lime, and has for its representative formula CS^2.—*Bonsdorff, Mem. Acad. Petersb.* 1825.

16. *Notice regarding Steatite or Soap-Stone, and its principal uses.*—Steatite is, as is well known, a variety of the talc genus. Its colour is white, green, or grey; it is also sometimes, though rarely, red and yellow. Its specific gravity varies from 2.60 to 2.66. It is a compound of silica, alumina, magnesia, oxide of iron and water, which vary according to the locality. It is very common in Cornwall and Germany. As it is fusible only at an exceedingly high temperature, and is easily wrought, excellent crucibles may be made of it, which are further hardened by fire, and which are only with great difficulty penetrated by litharge. It is also employed in making moulds for melting metals. In England it is used in the manufacture of porcelain. M. Vilcot, an artist of Liege, made several trials of it with the view of finding out whether it might not be susceptible of being employed by the lapidaries. He prepared cameos of this substance, the colour of which he brightened in the fire, and which he rendered so hard by the elevation of the temperature, as to give sparks with steel. They were then coloured, yellow, grey, or milk-white, by different solutions. He polished them upon the stone, and ended with making them assume all the lustre of agate. Some pieces even resembled onyx in colour; but a serious inconvenience was, that the markings were easily altered by the fire, and could no longer be restored. Steatite has a great affinity for glass; it is also employed, in the manner of paste, reduced to a fine powder, and mixed with colouring matters, for painting upon this substance. It also serves as a sympathetic

crayon for writing upon glass; the traces seem effaced, when a piece of woollen cloth is passed over them, but they re-appear immediately when moistened by the breath, and again disappear when the glass becomes dry. Steatite is not so easily effaced as chalk, and does not, like that substance, change its colours. Tailors and embroiderers also prefer it to chalk, for marking silk. It possesses the property of uniting with oils and fat bodies, and enters into the composition of the greater number of the balls which are employed for cleaning silks and woollen cloths; it also forms the basis of some preparations of paint. It is employed also for giving lustre to marble, serpentine and gypseous stones. Mixed with oil, it is used to polish mirrors of metal and crystal. When leather, recently prepared, is sprinkled with steatite, to give it colour, and afterwards, when the whole is dry, it is rubbed several times with a piece of horn, the leather assumes a very beautiful polish. Steatite is also used in the preparation of glazed paper; it is reduced to very fine powder, and spread out upon the paper; or it is better to mix it previously with the colouring matter. The glaze is then given to the paper with a hard brush. It facilitates the action of screws, and from its unctuosity, may be employed with much advantage, for diminishing the friction of the parts of machines which are made of metal.

GEOLOGY.

17. *Professor Buckland's Notice of the Hyœnas' Den near Torquay.*—Professor Buckland has lately sent to Professor Jameson, for the College Museum, several specimens of bone from the hyenas' den at Kent's Hole, near Torquay, all of which he considers as bearing most decided marks of teeth and gnawing upon them. Three of these bones (Nos. 4, 5, 6.) are splinters, which appear to have been gnawed and nibbled over and over again, after they were split off from the cylindrical bones, of which they formed a part. Other splinters have not been gnawed after such fracture; but of these none have been sent at present,—Professor Buckland's sole object being to produce conviction in those who deny the fact of the marks of teeth and gnawing being visible on the bones found in our English hyenas' dens. Numbers 1, 2, 3, are portions of cylindrical bones, from which both extremities or condyles have been gnaw-

ed off at a period antecedent to that when they, as well as the splinters and teeth that accompany them, were imbedded in the mud and gravel that now surround them. Of more than a thousand bones, or rather fragments of bones, that have been collected recently in Kent's Hole, not fifty have been found entire. The condyles, and softer portions, are almost uniformly removed, and marks of gnawing and fracture, such as appear in Nos. 1, 2, and 3, are generally visible at the extremities of the remaining central and harder portions. The condition of the teeth,—the number and variety of animals,—and the circumstances that accompany their mangled remains, are precisely the same as at Kirkdale; the only difference is, that at Torquay, the cave is more than twenty times as extensive as that in Yorkshire; and the remains of all kinds, nearly in the same proportion more numerous. The superficial crust of stalagmite, and the bed of mud which forms the matrix of the broken bones and teeth beneath it, are also in the same proportion thicker. There are also *album græcum*, as at Kirkdale, and stumps of gnawed horns of deer; and the bony bases of horns of rhinoceroses, but no *horns of this animal*, although more than a hundred of its teeth have been already found; also the teeth of many infant elephants,—numberless bones of horses, elks, deer, and oxen, —and gnawed bones and jaws of hyenas, with their single teeth and tusks; also the teeth and tusks of bears, tigers, wolves and foxes,—and of an unknown carnivorous animal, at least as large as a tiger; the genus of which has not yet been determined. All these will be described in Professor Buckland's second volume of Reliquiæ Diluvianæ. The history of the Torquay cave being, according to Professor Buckland, identical with that of Kirkdale, is totally different from that of the cavernous fissures at Plymouth and Banwell; both the latter containing bones that are usually entire, and never gnawed; and which appear to have been supplied by animals that fell into the open fissures, before they were filled with the mud and gravel that now envelope their bones *.

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* These bones were exhibited at a late meeting of the Wernerian Society, when several of the members agreed in considering the furrows on the bones, as very probably produced by the teeth of some quadruped.—Edit.

ZOOLOGY.

18. *On the Serpents of Southern Africa.*—" I have made a great many experiments upon such serpents as I have been able to procure alive, and have thereby ascertained which of them are, or are not, poisonous. I always feel a great degree of surprise, when I consider how little this branch of Natural History has been attended to; and how very vague and unsatisfactory our knowledge is, relative to the whole Linnæan class Amphibia. One would almost fancy, that next to the animals particularly useful to man, they would have been studied, in consideration of the consequence attached to them, from the peculiar powers which some of them possess. That, however, is far from being the case; and the neglect with which these animals have been treated, is probably to be attributed to the dread and disgust with which the whole tribe are viewed; feelings, however, which are both increased and diminished by experiments, inasmuch as by them we discover beyond doubt the mortiferous power of some, and to an equal certainty the innocence of the majority. So little is yet known of the snakes of this colony, that, at the present moment, nearly all are considered as poisonous; while, by actual experiments, I have found, that not a greater proportion than one to six of the species found here are noxious. We have three species of the viper, the bites of all of which are bad, though not invariably fatal; also three species of Naia, the bites of all of which produce almost certain death; and two species of Elaps, which, from my observations, are also very dangerous."—*Letter to Professor Jameson from Mr Thomas Smith, Museum, Cape-Town.*

19. *Mode followed by the Serpent-eater* (Falco Serpentarius) *for destroying Serpents.*—Before concluding (Mr Smith remarks), I may mention a curious circumstance, of which I was informed a few days ago, by a gentleman, upon whose veracity I can place the utmost dependence, and which is a fact, in as far as I know, not generally known. It relates to the mode which the Falco Serpentarius of Linnæus follows in destroying snakes. Some time ago, when the said gentleman was out riding, he observed a bird of the above mentioned species, while on the wing, make two or three circles, at a little distance from the spot on which he then

was, and after that suddenly descend to the ground. On observing the bird, he found it engaged in examining and watching some object near the spot where it stood, which it continued to do for some minutes. After that, it moved with considerable apparent caution, to a little distance from the spot where it had alighted, and then extended one of its wings, which it kept in continual motion. Soon after this artifice, the gentleman remarked a large snake raise its head to a considerable distance from the ground, which seemed to be what the bird was longing for, as the moment that took place, he instantly struck a blow with the extremity of the wing, by which he laid his prey flat on the ground. The bird, however, did not yet appear confident of victory, but kept eyeing his enemy for a few seconds, when he found him again in action, a circumstance that led exactly to a repetition of the means already detailed. The result of the second blow appeared, however, to inspire more confidence; for almost the moment it was inflicted, the bird marched up to the snake, and commenced kicking it with his feet; after which, he seized it with his bill, and rose almost perpendicularly to a very considerable height, when he let go the reptile, which fell with such violence upon the ground, as seemingly to satisfy him, that he might now indulge himself with the well-earned meal in perfect safety."

20. *Remarks on some Marine Fishes, and on their Geographical Distribution. By MM. Quoy and Gaimard.*—This memoir is a general account of the observations which these two naturalists have made, during the voyage of the corvette Urania round the World. It will contribute to throw some light upon the hitherto little investigated manners of the fishes which inhabit the vast solitudes of the ocean, and will serve as a point of departure, for connecting one day the observations which long voyages cannot fail to furnish to the attentive observer. Fishes, in fact, from the nature of the element which they inhabit, are more imperfectly known than the other classes of organized beings which are more easily subjected to investigation. But a real obstacle, which will long prove detrimental to the advancement of Ichthyology, is the little time which naturalists can devote on voyages to this study, in the richest and least known seas. Some general data are ably developed by our authors; who,

besides, trace the limits or the parallels which certain fishes affect. At the head of the species which roam at large through the solitudes of the ocean, they place the shark, giving new accounts of it, foreign to the popular histories, to which certain navigators have given their assent. They think, contrary to the opinion of M. Noël de la Moriniere, that the *Squalus Carcharias* inhabits every sea that they have visited. Speaking of the Coryphenes and Scombri, they exhibit to us the swarms of these voracious fishes plowing the seas in all directions, without fixed limits. Then, passing to the equatorial zones, they paint the brilliancy and richness of colouring which nature has imparted to the species which live in the midst of the coral-reefs, where they rival, in the vivacity and the delicate blending of their tints, the purest and most brilliant productions of the vegetable kingdom. Of this kind are the *Chætodons*, *Glyphisodons*, *Pomacentri*, *Acanthuri*, &c. On the other hand, in the places where the waves dash with fury upon the rocky shores, there live by preference, the tribe of the *Balistes*, the *Labroides*, the *Somphoses*, *Diacopi*, *Scari*, and *Caranges*. But in all, according to our authors, gold and silver mingle their hues with the prismatic tints; everywhere in the torrid zone, the same phenomenon manifests itself. They also affirm, that the descriptions of Renard, which were so long supposed to be the products of imagination rather than the result of actual existence, are perfectly correct with regard to the marvellous reflections of colour; and that if there be errors in the case, they exist in the representation of the forms. But, in proportion as we recede from the zone, which is constantly warmed by torrents of heat, the rich livery of certain beings disappears, and gives place to duller tints. It is chiefly the fishes of New Holland, Port-Jackson, the Cape of Good Hope, the Rio-de-la-Plata, that are adduced as examples, although this modification of life experiences numerous exceptions even in our own countries. Rio Janeiro, placed under the tropic, forms an exception to this rule however, and the most common fishes have dull colours, and are in general *Rays*, and several species of the family of *Salmones*, such as the *Currniates*, *Hydrocynes*, &c. The Volcanic Sandwich Islands are chiefly peopled with *Labroids*, which again appear not to have adopted the coasts of the Moluccas and Marian

Islands, although abounding in corals and plants. Lastly, They indicate, in concluding, both the fishes, which, wandering from their native haunts, follow ships, sheltering themselves under their keel; and those which various navigators have fallen in with in thick shoals in a dead state, and destroyed by causes still little known. This memoir, the result of observations full of sagacity, will be most highly appreciated by those who have had an opportunity of judging on the spot of the facts which they have described with accuracy.—*Ann. des. Sc. Nat.*

BOTANY.

21. *Original Habitats of the Rose.*—In Trattinick's Synodus Botanica, it is mentioned, that the species of the genus Rosa found in Europe, have reached us from the East Indies, China, and Japan. The middle part of the Russian empire, the districts around Caucasus and Persia, are full of roses, of which the more western are mere varieties, and which have propagated themselves as such. Roses are rare in Africa; there they are met with only in the northern districts; while Europe, on the contrary, from the Uralian Mountains to the coast of Portugal, abounds with them. The roses of America have reached that continent through the Polar lands, and appear to be sprung from the Rosa Alpina, and R. Majalis. There are no roses in Australasia, nor have any species been met with in South America; indeed, they scarcely occur any where to the south of the Equator.

22. *Number of Species of the Genus Rosa.*—Willdenow, in his Species Plantarum, published in 1800, enumerates 39 species of Rose; Persoon, in his Enchiridium Botanicum, increased the number to 45; Trattinick, in his Synodus Botanica, published in 1824, enumerates 206 species; and since the appearance of that work, late discoveries make the total number of known species 240. These are divided into 24 series, each of which bears the name of some botanist, who has distinguished himself by his knowledge of this beautiful genus. Thus we have as names, the following:—1. Jacquinia; 2. Smithiana; 3. Candolleana; 4. Willdenowiana; 5. Woodsiana; 6. Sprengeliana; 7. Linkiana; 8. Andrewsiana; 9. Purshiana; 10. Lindleyana; 11. Aitoniana; 12. Pallasiana, &c.

23. *Notice regarding the Boletus igniarius.*—An individual plant of *Boletus igniarius* was remarkable for its enormous size, and the fleshy nature of its substance. After a large circular incision had been made in it, the two edges were united by the first intention, and were readily consolidated. Still farther, a portion of the fungus cut off and left on the ground for two days, was applied to a newly cut portion of the Boletus. The union took place as well as in the former case; and the separated part could only be known by the cicatrix.—*Amer. Journ. of Sciences and Arts.*

24. *Naturalization and cultivation of the Larger-fruited Vaccinium.*—Various species of the genus Vaccinium are common in the woods and moist places of the north of Europe. The species known in France by the name of *Lucet*, in England Bilberry or Whortle-berry, and among botanists by that of *Vaccinium myrtillus*, occurs in the neighbourhood of Paris, in the wood of Montmorency. It is very common in Lorraine, where it is eaten in large quantities, especially by the poorer classes. Its fruit is much smaller than that of the large-fruited vaccinium. It is gathered in the woods, and eaten fresh, or it is preserved through the whole year, after having been dried in the sun, or in an oven, or even in the shade. The best manner of preparing it is in pastry. It is used in tarts, instead of cherries, gooseberries or prunes. It requires to have a little sugar added, to conceal the styptic or acrid taste peculiar to it. Some people season it with honey, others eat it in milk. It is also employed for making preserves, puddings, &c. It is of great use on voyages. It is used in Germany for colouring wines, and forms, in this respect, a considerable article of commerce. It is also steeped in *eau-de-vie.* The Laplanders esteem this berry highly; it is, however, much inferior to the *Rubus Chamæmorus*, which travellers mention their having eaten with much relish, during their stay among the Norwegian Laplanders. There are seven or eight species of vaccinia which furnish an article of food to man, besides being applied to other economical purposes; but the species whose cultivation has been introduced into England, is in every respect preferable to the others. It is designated by botanists under the name of *Vaccinium macrocarpum:* its fruit was long known to the English, who annually brought a considerable quantity of

it from North America, for internal consumption, as well as for the use of the navy. The large-fruited American vaccinium has been successfully cultivated at Spring-Grove, the country house of the late Sir Joseph Banks, near London, for several years. This shrub produced flowers and fruit the first year, and the quantity obtained the following harvest was still more abundant. It gradually threw out spreading roots like those of the gooseberry, but longer, and which took with more difficulty; they succeeded, however, and afforded at the proper time in spring, branches from ten to twelve inches long, with flowers. The berries were gathered, and were found excellent, and much superior to those commonly imported. The ground employed for this purpose was 326 square feet, while the quantity used for the cultivation of gooseberries in Spring-Grove garden, is 5,646 square feet, deduction made of the spaces left between each row. It is to be remarked, that the harvest of these berries has been constantly abundant for seven successive years, without having been damaged by the vicissitudes of the weather, by mildew, or by any other accident. The flowers, which have expanded abundantly in the season, have been blasted in much smaller number than in the other species of plants. The fruit has been developed, and has acquired its full maturity, without being attacked by insects, and without suffering from excess of cold or heat, rain or dryness *.

ARTS.

25. *Steam Navigation*.—While a great steam-vessel is crossing the Atlantic Ocean from the mouth of the Thames to the mouth of the Ganges; while other English vessels of the same description are intended to establish communications between Alexandria and the Island of Malta, several undertakings of a like nature, although not so extensive, are daily tending to give a greater activity to the navigation between the trading ports, upon the lakes and in the internal seas of Europe. A steam-boat goes from Hamburg to London in sixty hours: Another navigates between Kiel and Copenhagen, across the Baltic: A company is forming at Copenhagen, at this moment, for establishing a

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* The fruit of the *Vaccinium oxycoccos* (Cranberry) is, in the opinion of many, superior as an article of domestic use to that of the *V. macrocarpum*, and Mr Mylne of the Fulham Nurseries, has found that it is easily susceptible of garden cultivation.

steam-boat upon the Kategatt: A steam-boat navigates the Gulf of Finland, between the capitals of Russia and Sweden: A boat of a new construction has arrived at Stockholm, in order to be employed upon the great lakes which open to Sweden a navigation, independent of the passage of the Sound. The trial of a steam-boat upon the Danube, between Vienna and Semlin, has not entirely answered; but it is believed that an improvement in the construction of the vessel will remedy the inconveniences which have been experienced. This communication will facilitate the commerce between Constantinople and all the northern parts of Turkey. The beautiful lakes of the Alps are beginning to be filled with steam-boats; those of the Lake of Constance are in full activity; that of the Lac Majeur is building. These vessels, and the new roads, will render twice as quick the communications between Augsbourg, on the one hand, and Milan and Genoa, on the other. An enterprise in which France is more directly interested, is that of the navigation from Mayence to Kehl. For the whole voyage from Rotterdam to Kehl, the following are the calculations of the times and distances:

From Rotterdam to Cologne,	37h.	30m.	59 leagues *.
Cologne to Coblentz,	14	10	19
Coblentz to Mayence,	13	53	21
Mayence to Manheim,	11	21	16
Manheim to Schroeck,	11	24	14
Schrock to Fort-Louis,	12	23	10
Fort-Louis to Kehl,	11	4	9
	111	45	148

26. *Method of using pure Muriate and Sulphate of Soda, in the Manufacture of Glass, by M. Leguay.*—Muriate of soda and sulphate of soda, may be employed, and at times with advantage, in glass-making. A casting is readily obtained of very fine glass, having, when about three or four lines in thickness, a very slight green tinge. Its composition is as follows: decrepitated muriate of soda, 100 parts; slaked lime, 100; sand, 140; clippings of glass, of the same quality, from 50 parts to 200. Sulphate of soda likewise offers a great economy in its employment. The results are very satisfactory. The glasses made with this salt were of a very fine quality. The following is the composi-

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[*] Leagues of 25 to a degree.

tion : dry sulphate of soda, 100 parts ; slaked lime, 12 ; powdered charcoal, 19 ; sand, 225 ; broken glass, from 50 to 200. These proportions give a rich coloured glass, which may be employed with advantage in glass-houses, where a fine quality is sought after. The following is the second way of operating with sulphate of soda ; the proportions may be as follows : dry sulphate of soda, 100 parts ; slaked lime, 266 ; sand, 500 ; broken glass, from 50 to 200. According to this process, it is obviously easy to operate in a regular manner, and to avoid expensive trials in the manufacture.—*Annales de l'Industrie Nationale.*

27. *On the advantages of improving the qualities of Cutting Instruments, by Burnishing, and thereby condensing their edges.* By THOMAS GILL, Esq.—The *condensing process* of *hammer-hardening* the edges of cutting-instruments, such as the graver and the scythe, has naturally led us to consider the action of the burnisher upon the edges of other cutting-instruments in a similar light ; and to infer that a great part of the benefit derived therefrom must be owing to its *condensing effect*, as well, also, to its giving the edges a more favourable position for effecting the different purposes they are applied to. The *currier's shaving knife* is the first instance we shall quote, where, after renewing its edge, by whetting it upon the proper whet-stone, as well as continually during its use, the edge is always burnished. The next, and a familiar example, is in the steel-scraper used by the cabinet-makers to smoothen the surface of hard wood after the toothed plane, previous to varnishing or polishing it. When the edge of this hardened and tempered flat piece of sheet-steel becomes dull, it is renovated by placing it upright, and whetting it upon the oil-stone ; it is then whetted upon each side, to remove the burs ; and, lastly, burnished upon the face of it, towards each side, so as to throw the edges outwards. It is held in a sloping direction in use, exactly as a piece of broken window-glass is held when used as a shave, for which, however, it is an admirable substitute, as it performs its work in a similar, though much more perfect, manner. The next example is furnished from the practice of a late ingenious mathematical instrument-maker, Mr R. Fidler, who was continually employed by the late Mr W. Lowry, the celebrated engraver, when he had any instruments to be made, for his business of mechanical

engraving, which required particular accuracy in their construction. He was in the habit of finishing his turning-tools for brass, after forming them into shape, and whetting them, by burnishing their edges from their sides toward their flat faces, and thus giving them a hardness and smoothness not to be acquired in any other way; and, in fact, they polished the brass-work turned by them. The last instance is borrowed from the practice of a late eminent mechanic in this country. He was employed to make some hardened and tempered steel-cutters for an engine, and which were to be driven with great velocity by a steam-engine at Manchester, for a cotton-mill there, to cut brass-toothed wheels and pinions, they requiring to be cut, rounded off, and polished at once. After properly shaping them, and skive-grinding the faces of their teeth, he finished them by burnishing their edges from their sides to their flat faces; and their effects in cutting and polishing the teeth at once were truly wonderful.—The *currier's shaving-knife* is a two-edged instrument, about 3½ or 4 inches broad, 14 inches long in the blade, and half an inch thick in its middle part, gradually tapering away from thence to its edges. It has two handles, one in the direction of the blade, and the other at right angles to it. It requires to be made of excellent steel, and to be well tempered; and, indeed, there are but very few makers of repute in this country. When the edge requires grinding and whetting, the former of these operations is performed upon a flat rub-stone, similar to what carpenters sharpen their plane-irons on, with the application of water. This stone is about 6 inches broad and 18 inches long; and, so very careful are they to keep its surface flat, that it is a regulation in the work-shops, for every workman, after using the stone, to write his name upon it with a piece of coal; when, if his successor finds it left so uneven that a halfpenny can be passed underneath the edge of an iron laid upon it, the former workman is subjected to a fine for his carelessness. After being carefully ground upon this stone, it is whetted upon a flat circular piece of Welsh or Scotch blue-stone, about 8 inches in diameter; likewise with the application of water, carefully preserving the edges straight. The edges are then ready to receive the effect of the burnisher upon them, to turn them to the two opposite sides, and fit them for use. The *burnisher* consists of a hardened and polished steel-wire, having its end made hemispherical, mounted into a handle of hard wood. This delicate instrument is ordinarily held by the small part of its handle, which is terminal, between the third and little fingers of the workman's right hand, ready for use at each cutting-stroke, or shave made upon the wet leather by the knife, to renew its edge, first by raising it, then by passing the hemispherical end of the burnisher along it, and then to turn over and give it its proper direction for use, with the cylindrical part of it likewise passed lightly along it.—The application of the burnisher to the edge of that useful and necessary instrument, the *pen-knife*, is equally advantageous as in the former case. If the blade be first whetted, with care, in the ordinary manner, and the edge then finished by a gentle and delicate stroke of the burnisher, carried along it so as to throw it forward a little from the back or convex side of the blade toward the concave side, a great improvement is effected; and the edge, thus perfected, will endure for a considerable time.—*Tech. Repos. Nov. & Dec.* 1825.

28. *On the French mode of Treating Scythes by hammering them cold.*—On Mr Gill's mentioning Mr Turrell's great improvement in gravers (recorded in pages 196, 197, and 198. of this volume), to the person who furnished the notice respecting the French method of treating scythes, inserted in the 3d volume of the Technical Repository, namely, by placing the scythe *flatways* upon a portable anvil, fixed in the head of a stake driven into the earth, and hammering its edge dexterously all along it with gentle strokes, he immediately noticed the very great analogy in the two methods, though applied in a different manner, and to very different purposes. Mr Turrell's great success in the improvement of that highly important implement the graver, fully warrants the conclusion, that the scythe may likewise be greatly improved by the condensing effect of the blows of the hammer upon the flat sides of its edge. Thus the one improvement throws an additional light upon the other; and we shall gladly learn the success of the application of this valuable practice of *hammer-hardening in the cold*, after the usual hardening and tempering processes, to such objects as it may, and no doubt will, now be very shortly employed upon.—*Gill's Technical Repository*, *Nov.* 1825.

29. *On improving Bricklayers' Trowels, by hammer-hardening them; by Mr Walby.*—There is not, perhaps, an implement that undergoes more severe treatment than this, in its constant employment of hacking bricks into shape, and thus encountering the pieces of flint, pebbles, &c. ordinarily mixed with the clay; and which, besides having a tendency to injure its edges, also render it liable to break continually. Mr George Walby, therefore, by his excellent processes, accomplished a most difficult task, and rendered his trowels highly prized, by those who were the most competent judges of their merit, from their constant experience in their use. They were made of the best shear-steel, carefully worked throughout, and especially to avoid over heating the steel; and towards their finishing in the plating or forging, and when nearly reduced to their proper thickness, besides heating them in a clean hollow fire, to avoid contact with cinders, &c.; he also removed all scales upon their surface, previous to giving them their last polishing, under the rapid blows of a hammer driven by a steam-engine, by means of a very ingenious revolving elastic steel-brush of his invention. He carefully attended to the proper hardening heat, and quenched them in a composition or hardening liquor, similar to those used by saw-makers; he next blazed them off to the spring temper, and, lastly, hammer-hardened them as much as possible. They were then ready for grinding; after which operation, their elasticity being again restored by blueing them, they were glazed or brightened, ready to be mounted into their handles.—*Gill's Technical Repository.*

30. *On improving Drills by hammer-hardening them cold.*—Mr Andrew Pritchard, the inventor of the hard shell-lac cement, finding that steel, when hardened and tempered, is susceptible of receiving the condensing effect of the hammer, has applied it, with considerable advantage, to the points of small drills, by hammering them upon their flat surfaces.

31. *On the improvement of Square Broaches* or *Boring-bits.*—Mr Joseph Clement, an excellent workman and mechanical draughtsman, informed Mr Gill, that a friend of his in Scotland, many years since, improved the quality of his square broaches, by hammer-hardening them cold, after being hardened and tempered upon their flat sides. Mr Gill thinks it would have been

much better to have hammered them upon their angles, which would have had a much greater condensing effect.—*Technical Repository, December.*

32. *Blue and Green Colours derived from Althæa rosea.*—M. Bauhart, apothecary at Weimar, has discovered an easy process for obtaining a beautiful blue from the leaves of *Althæa rosea* of Willdenow. The flower furnishes a very beautiful green, which may be used for dyeing wool, wood, horn, &c. The blue colour obtained from the leaves is averred not to be inferior to indigo. Nothing is said of the modes used by the discoverer for extracting the colours.

33. *Melaina.*—Signior Bizio considers the black matter of the ink of the cuttlefish, as a substance *sui generis*, which he calls *Melaina*, from μέλας and ἀεὶ. It is obtained by digesting the ink with very dilute nitric acid, until it become yellowish, washing it well, and separating it by the filter; it is then to be frequently boiled in water, one of the washings to be a little alkalized, and, finally, with distilled water. The Melaina is a tasteless, black powder, insoluble in alcohol, ether, and water, while cold, but soluble in hot water; the solution is black. Caustic alkalies form with it a solution even in the cold, from which the mineral acids precipitate it unchanged. It contains much azote. It dissolves in and decomposes sulphuric acid. It easily kindles at the flame of a candle. It has been found to succeed as a pigment, in some respects better than china ink. —*Dub. Phil. Journ., Nov.* 1825.

34. *New method of preparing Quills.*—The following is the manner in which M. Schloz of Vienna proceeds in the preparation of quills for writing, by means of which he renders them more durable, and even superior to the best Hamburgh quills. For this purpose he makes use of a kettle, into which he pours common water, so as to occupy the fourth part of its capacity; he then suspends a certain quantity of feathers perpendicularly, the barrel lowermost, and so placed, as that its extremity only may touch the surface of the water; he then covers the kettle with a lid properly adjusted, boils the water, and keeps the feathers four hours in this vapour bath. By means of this process he frees them of their fatty parts, and renders them soft and transparent. On the following day, after having scraped

them with the blade, and then rubbed them with a bit of cloth, he exposes them to a moderate heat. By the day after, they are perfectly hard and transparent, without, however, having the inconvenience of splitting too easily.—*Neues Kunst und Gewerb-Bl. April* 1825.

35. *Panto-chronometer.* — This interesting little instrument is a combination of the compass, the sun-dial, and the universal time-dial; and therefore unites, to a certain extent, the uses of the three. It is a beautiful, at the same time a useful toy, and cannot fail to engage the attention, and excite the curiosity, of young persons, for whose use it is intended.

COMMERCE.

36. The following Table gives an interesting view of the present flourishing state of the maritime capital of the Pacha of Egypt.

Number of Vessels arrived at the Port of Alexandria in the years 1822, 1823, *and* 1824.

	1822.	1823.	1824.
Austrian and Tuscan vessels, - -	292	351	600
Danish, - - - -	15	25	13
French, - - - -	57	52	111
English, American, Ionian, - -	223	230	251
Roman, - - - -			2
Russian, - - - -	10	59	100
Sardinian, - - - -	143	98	77
Dutch, - - - -	3	1	5
Spanish, - - - -	54	24	70
Swedish - - - -	76	81	47
Sicilian, - - - -	28	12	14
Total,	901	933	1,290

There sailed from it in 1824:

For Amsterdam,	4	For Genoa,	53
Antwerp,	1	Hull,	1
Dublin,	1	Liverpool,	30
Gibraltar,	4	London,	21
Leghorn,	102	Petersburg,	2
Marseilles,	97	Rotterdam,	1
Malta,	57	Trieste,	57
Port Mahon,	20	Venice and Fiume,	9

STATISTICS.

37. *Population.*—In Great Britain, the number of individuals in a state to bear arms, from the age of 15 to 60, is 2,744,847. The number of marriages is about 98,030 yearly; and it has been remarked, that in 63 of these unions there were only 3 which had no issue. The number of deaths is about 332,708 yearly, which makes nearly 25,592 monthly, 6398 weekly, 914 daily, and 40 hourly. The deaths among the women are in proportion to those of the men as 50 to 54. The married women live longer than those who continue in celibacy. In the country, the mean term of the number of children produced by each marriage is 4; in towns the proportion is 7 for every two marriages. The number of married women is to the general number of individuals of the sex as 1 to 3; and the number of married men, to that of all the individuals of the male sex, as 3 to 5. The number of widows is to that of widowers as 3 to 1; but the number of widows who marry again, is to that of widowers in the same case, as 7 to 4. The individuals who inhabit elevated situations live longer than those who reside in less elevated places. The half of the individuals die before attaining the age of 17 years. The number of twins is to that of ordinary births as 1 to 65. According to calculations founded upon the bills of mortality, one individual only in 3126 attains the age of 100 years. The number of births of the male sex is to that of the female sex as 96 to 95.

ART. XXVI.—*List of Patents sealed in England from* 17*th November* 1825 *to* 23*d January* 1826.

1825.

Nov. 24. To **Augustus** Count de la Garde, of St James's Square, London; who, in consequence of a communication made to him by a certain foreigner, residing abroad, is in possession of certain improved Machinery for Breaking or Preparing Hemp, Flax, and other Fibrous Materials.

To **Joseph Eve**, of Augusta, Georgia, in the United States of America, but now residing at Liverpool, engineer; for "an improved Steam-Engine."

26. To **Henry King**, of Norfolk Street, Commercial-Road, London, master-mariner; and **William Kingston**, of the Dock-yard, Portsmouth, master-millwright; for "certain improved Fids for

Topmasts, Gallantmasts, Bowsprits, and all other Masts and
25. Spars, to which the use of the Fid is applied."

28. To **Mark Lariviere**, of Prince's Square, Kennington, in the county of Surrey, mechanist; for "certain Apparatus or Machinery, to be applied to the well-known Stamps, Fly Presses, or other Presses, for the purposes of Perforating Metal Plates, and for the application of such perforated metal plates to various useful purposes."

3. To **William Pope**, of Ball-ally, Lombard Street, London, mathematician; for "certain improvements on Wheeled Carriages."

To **William Pope**, of Ball-ally, Lombard Street, London, merchant; for "an improved method, in different shapes or forms, of securing volatile and other fluids, and concrete or other substances, in various descriptions of Bottles and Vessels."

To **Ezekiel Edmonds**, of Bradford, in the county of Wilts, clothier; for "certain improvements on machines for Scribbling and Carding Sheep's Wool, Cotton, or any other fibrous articles requiring such process."

To **John Beever**, of Manchester, in the county of Lancaster, gentleman; for "an improved Gun-barrel."

6. To **Edmond Luscombe**, of East Stonehouse, in the county of Devon, Merchant; who, in consequence of communications made to him by a certain foreigner, residing abroad, and discoveries made by himself, is in possession of a method of manufacturing or preparing an Oil, or Oils, extracted from certain vegetable substances, and of the application thereof to Gas-light, and other purposes.

7. To **John Phillips Beavan**, of Clifford Street, London, gentleman; who, in consequence of communications made to him by a certain foreigner, resident abroad, is in possession of an invention of a Cement, for building and other purposes.

8. To **Francis Halliday**, of Ham, in the county of Surrey, Esq.; for "certain improvements in Machinery, to be operated upon by Steam."

ec. 9. To **Joseph Chesseborough Dyer**, of Manchester, in the county of Lancaster, patent card manufacturer; for "certain improvements in machinery for making Wire Cards, for carding Wool, Cotton, Tow, and other fibrous substances of the like nature; and also, certain improvements on a machine for shaving and preparing leather, used in making such Cards."

14. To **Robert 'Addams**, of Theresa-Terrace, Hammersmith, in the county of Middlesex, gentleman; for "a method of propelling or moving Carriages, of various sizes, on turnpike, rail, or other roads."

To **Matthew Ferris**, of Longford, in the county of Middlesex, calico-printer; for "improvements on presses, or machinery, for printing Cotton and other Fabrics."

To **James Ashwell Tabor**, of Jewin Street, Cripplegate, London,

gentleman; for "means for indicating the Depth of Water in
1825. Ships and Vessels."
Dec. 27. To John Maccurdy, Esq. London; for "certain improvements
1826. in generating Steam."
Jan. 6. To James Oyston and James Thomas Bell, of London, watchmakers; who, in consequence of a communication made to them by a certain foreigner, residing abroad, are in possession of certain improvements in the construction or manufacture of Watches of different descriptions.

7. To Richard Evans, of London; for "certain improvements in the Apparatus for, and process of, Distillation."

16. To Henry Houldsworth *jun.* of Manchester, cotton-spinner; for "certain improvements in machinery for giving the taking up, or winding on, motion to spools, or bobbins, and tubes, or other instruments, on which the roving, or thread, is wound, in roving, spinning, and twisting-machines."

To Benjamin Newmarch, of Cheltenham, Esq.; for "an improved method of Exploding Fire-arms."

To John Rothwell, of Manchester, tape manufacturer; for "an improved Heald, or harness, for Weaving."

To Henry Anthony Koymans, of London, merchant; who, in consequence of certain communications made to him by a certain foreigner, residing abroad, is in possession of certain improvements in the construction and use of apparatus and works for Inland Navigation.

17. To William Whitfield, of Birmingham; for "certain improvements in making or manufacturing of handles for saucepans, kettles, and other culinary vessels; and also, Tea Kettle Handle Straps, and other articles."

19. To John Frederick Smith, of Dunstan Hall, Chesterfield, in the county of Derby, Esq.; for "an improvement in the process of drawing, roving, spinning, and doubling Wool, Cotton, and other fibrous substances."

19. To Benjamin Cook, of Birmingham, brass-founder; for "certain improvements in making or constructing Hinges, of various descriptions."

To Abraham Robert Cohent, of Gottenburgh, merchant; at present residing in London; for "a method of applying steam, without pressure, to pans, boilers, coppers, stills, pipes, and machinery, in order to produce, transmit, and regulate various Temperatures in the several processes of boiling, distilling, evaporating, inspissating, drying, and warming, and also to produce power."

To Sir Robert Seppings, London; for "an improved construction of such masts and bowsprits, as are generally known by the names of Made Masts, and Made Bowsprits."

23. To Robert Stephenson, of Bridge Town, Warwickshire, engineer; for "his Axletrees, to remedy the extra friction on curves to waggons, carts, cars, and carriages, used, or to be used, on railroads, railways, and other public roads."

ART. XXVII.—*List of Patents granted in Scotland from 17th November* 1825 *to* 16*th February* 1826.

1825.

Nov. 23. To **Alexander Lamb** of Prince's Street Bank, London, gentleman, and **William Suttill** of Old Brompton, county of Middlesex, flax-spinner, for "Improvements in Machinery for preparing, drawing, roving, and spinning Flax, Hemp and Waste Silk."

Dec. 14. To **John Gotlieb Ulrick** of Upper Rosamond Street, Clerkenwell, county of Middlesex, chronometer maker, for "certain Improvements in Chronometers."

15. To **John Maccurdy** of Cecil Street, Strand, county of Middlesex, for certain "Improvements in generating Steam."

1826.

Jan. 4. To **Edmund Luscombe** of East Stonehouse, county of Devon, merchant, for "a New Method of manufacturing or preparing an Oil or Oils, extracted from certain vegetable substances, and the application thereof to Gas Light and other purposes."

4. To **Ezekiel Edmunds** of Bradford, county of Wilts, clothier, for "certain Improvements on Machines for scribbling and carding Sheep's Wool, Cotton, or any fibrous articles requiring such process."

4. To **Joseph Chesseborough Dyer** of Manchester, county of Lancaster, patent card manufacturer, for "a Method of conducting to and winding upon Spools or Bobbins, rovings of Cotton, Flax, Wool, or other fibrous substances, communicated by a foreigner residing abroad."

18. To **Moses Poole** of the Patent Office, Lincoln's Inn, county of Middlesex, gentleman, for "the Preparation of certain substances for making Candles, including a Wick peculiarly constructed for that purpose, communicated by a foreigner residing abroad."

1826.

Jan. 18. To **John Harvey Saddler**, late of Hoston, county of Middlesex, now of Broadwall, county of Surrey, mechanist, for "an Improved Power-Loom for the weaving of Silk, Cotton, Linen, Wool, Flax and Hemp, and all mixtures thereof."

18. To **John Stephen Langton** of Langton, near Partney, county of Lincoln, Esq. for "Methods of seasoning Timber."

30. To **James Blyth Waynman** of Brunswick Place, City Road, county of Middlesex, gentleman, for "Improvements in the manufacture of Hat-bodies, communicated by a foreigner residing abroad."

Feb. 1. To **Thomas Cook** of Upper Sussex Place, Kent [illegible] Road, county of Surrey, Lieutenant in the Navy, for "Improvements in the construction of Carriages, and other Harness to be [illegible] used therewith, whereby greater safety to the persons riding in [illegible] such carriages, and other advantages, will be obtained."

Feb. 1. To **Thomas Woolrich Stansfeld**, merchant, and **William Prichard**, civil-engineer, both of Leeds, county of York, for "Improvements in Looms, and in the implements connected therewith."

1. To **Goldsworthy Gurney** of Argyll Street, Hanover Square, county of Middlesex, surgeon, for "an Apparatus for propelling Carriages on common roads or on railways."

2. To **James Brown**, paper-maker at Eskmills, parish of Penycuik, county of Edinburgh, for "a new Method of bleaching the pulp for making Paper."

10. To **Charles Freund** of Bell Lane, Spittalfields, county of Middlesex, sugar-refiner, for "an Improvement or Improvements in the process of refining Sugar."

11. To **Joel Lean** of Fish-pond House, near Bristol, gentleman, for "a Machine for effecting an alternating motion between bodies revolving about a common centre or axis of motion; also certain additional machinery or apparatus for applying the same to mechanical purposes."

11. To **Josias Christopher Gamble** of Liffybank, county of Dublin, chemist, for "certain Apparatus for the concentrating and crystallization of aluminous and other saline and crystallizable solutions, part of which apparatus may be applied to the general purposes of evaporation, distillation, inspissation and desiccation, and especially to the generation of Steam."

16. To **Nicolas Hegesippe Manicler**, of No. 102. Great Guildford Street, Southwark, county of Surrey, chemist, for "a new Preparation of fatty substances, and the application thereof to the purposes of affording light."

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